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A SCIENTIFIC QUARTERLY DEALING WITH EXTRASENSORY PERCEPTION, THE PSYCHOKINETIC EFFECT, AND RELATED TOPICS

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The Journal *of*
Parapsychology

Volume 8 MARCH, 1944 Number 1

EDITORIAL

THE PK RESEARCH AT THE POINT FOR DECISION!

WHEN the first report on the PK research was published a year ago, the critical reader was urged to suspend judgment on the PK hypothesis until he was acquainted with all the work that had been done. Now, however, the situation has changed, and suspended judgment is, we believe, no longer necessary. The issue of the PK hypothesis can be decided within the scope of a single article. We refer to the paper on the quarter distribution (QD) in this issue.

This abrupt departure from the earlier position is due to the fact that a superior order of evidence has been found which was not known at the time of the first report. In the analyses for the QD there have emerged evidences of lawfulness in hit patterns throughout so large a portion of the available experimental data that all reasonable doubt of the validity of the PK hypothesis has, we maintain, disappeared, and all counter-hypotheses seem to us manifestly out of the picture.

These statements are, we recognize, very strong, and they must seem to the reader who has not yet read the QD report very bold ones; but they are made with due deliberation and with what we regard as good reason. First of all, the QD paper referred to is but the beginning of a series of papers on quarter distributions. Each represents a check on the accuracy of the analyses in the others. Thus they form a highly and mutually confirmatory sequence of studies.

Most important of all is the fact that *these analyses can be repeated* as often as is desired until there is no question left regarding the accuracy and conclusiveness of the research. Already such a reanalysis has been made on most of the work reported in this first QD paper; Dr. J. G. Pratt has gone over the most important records involved. As his report in this issue indicates, he has found only very minor errors, and these in only one series. His work provides an independent confirmation.

The same type of reanalysis can, of course, be carried out by anyone qualified for the task. Indeed, such a recheck would be quite in order if it is generally felt that there is sufficient need of it, or if it might avoid a repetition of the relatively fruitless controversy which the ESP research evoked. We regard it as worth while, at least, to make it a matter of record that the Parapsychology Laboratory at Duke University is prepared to welcome the appointment of a suitable Investigatory Committee to examine the records and repeat the analyses on which the QD reports are based.

The revolutionary nature of the PK hypothesis certainly warrants every effort to bring the issue to an early decision; for, as the concluding remarks of the QD paper indicate, the PK hypothesis, if established, necessarily involves a shift of major consequence in the academic view of the nature of man, a shift which holds profound significance for our entire social philosophy. Today, men of larger social perspective are much concerned over freedoms, the practical freedoms that directly affect the individual's life and happiness. We suggest that these are dependent upon a deep respect for individual human worth and potentiality that will be lost if man comes to be regarded as a mere machine. In opposing the trend toward a physicalistic view of human personality, the researches which these pages have been reporting, and others like them, are, we believe, sustaining the common man's conception of freedom from mechanistic determinism, a conception on which the practical liberties and institutions of democracy would appear to depend.

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"PRECOGNITIVE TELEPATHY": THE SOAL AND GOLDNEY EXPERIMENTS

By J. G. Pratt

Introduction

Out of wartime England has come a report[1] of a parapsychological experiment which illustrates again that some of the most remarkable forward strides in pure science are made against the background of national or world crisis. There are several features of this Soal and Goldney report which make it especially noteworthy as a contribution to the scientific literature of this field of study. The authors have achieved such significant results under such carefully safeguarded conditions that any future critic who wishes to be taken at all seriously will have to deal with their report as one of his first and most urgent tasks. What is more, as far as the promise of positive contribution to the knowledge of extrasensory perception is concerned, their achievement ranks high among the published experiments. The research worker, however, will probably consider these results equally important for the questions which they raise and which the investigators have not answered. Finally, the subject, B.S., with whom all the tests were conducted showed such an array of remarkable peculiarities in the use of his ESP ability that, in my opinion, this contribution will prove to be one of the key pieces in the great puzzle of parapsychological phenomena.

In writing about this work, I have no intention of offering an adequate substitute for the original as far as the serious student of ESP is concerned. It is true that the full report was written with close attention to the rendering of a factually complete scientific account of the experiments, and the authors present a much larger mass of material than is necessary for an understanding of the essential points about the procedures and the results; but I wish it

[1] Soal, S. G., and Goldney, K. M. Experiments in precognitive telepathy. *Proc. S.P.R.*, 1943, **47**, 21-150.

clearly understood that this review will not spare any would-be critic the necessity of a full and painstaking reading of the original report. The present account is intended to be no more than a general story of these experiments and of the results, a story which is intrinsically interesting without any effort on the part of the teller to be sensational.

Mr. Soal reports that he first used B.S. as a subject in 1936 while he was conducting an extensive repetition of the Duke University experiments in ESP. The research was judged at that time to have yielded completely negative results, and it was not until 1939 that Soal was led, through a suggestion growing out of experiments by Mr. Whately Carington, to recheck the results of B.S. and of one other subject, Mrs. S., to see if there had been any *displacement* of hits;[2] that is, he re-examined these data to see if the subjects had scored more than a chance number of successes on the cards which came immediately before or immediately after the one in the deck which they had been trying to perceive. The former type of success is called "postcognitive" and the latter, "precognitive." Soal found conclusive evidence in this reanalysis of his earlier results that such displacement did occur in the records of these two subjects.

The investigations reported here were a continuation of the card-calling tests with B.S. The subject was a professional photographer, and, for his convenience, the tests were conducted in his studio. The experimental sessions generally occupied one evening of each week, though there were irregularities in the schedule, even to the point of discontinuing for a stretch of some months for the convenience of the subject. The whole investigation extended from January, 1941, to April, 1943. The following description of the procedure states the conditions which generally prevailed in the tests. These "standard" conditions were achieved after the first few sessions. From time to time throughout the investigations systematic changes were introduced in connection with different questions which were submitted to the test of experimental variations. These changes in procedure will be described in this digest in connection with the results to which they are relevant.

[2] Soal, S. G. Fresh light on card guessing—some new effects. *Proc. S.P.R.*, 1940, **46**, 152-98.

The Standard Experimental Procedure

Almost all of the tests were of the sort which the readers of this Journal know as GESP; that is, they were characterized by the fact that the subject was trying to name correctly the card upon which an agent was concentrating his attention. The testing material in these experiments differed in two important respects from Soal's earlier work. In the first place, five animal pictures (lion, elephant, zebra, giraffe, and pelican), reproduced in appropriate colors on the faces of cards, were used in the tests instead of the usual ESP symbols. Secondly, the agent used only five cards at a time, one of each picture, instead of the usual deck of twenty-five.

At the typical experimental session there were five people present. These were the agent, who looked at the cards; the subject, who attempted to identify the cards the agent was looking at; one experimenter who assisted the agent; another experimenter who watched the subject and sometimes recorded for him; and an observer who had been invited to witness the tests. For convenience, I will in this account refer to the investigator who assisted the agent as the *first experimenter* and to the person who watched the subject as the *second experimenter*.

All of the tests took place with the agent and the subject in adjoining rooms. The agent sat at a card table in the center of which was erected a screen slightly wider than the table. The screen reached from the table top to a height of twenty-six inches above it. In the center of this screen was an open window about three inches square. In addition, a box was placed on the card table with its only open side toward the agent. The data were kept on special forms which had spaces for the recording of two runs. At the beginning of each sheet of calls the agent would shuffle the five animal pictures and place them face-down in a row inside the box where they were visible only to himself. The positions of the cards were numbered one to five from left to right.

Meanwhile, the first experimenter (usually Mrs. Goldney) was seated at the card table opposite the agent and on the other side of the screen. In the first part of the investigations this experimenter had five cards, with the digits one to five, lying face-up on the card table. She also had a supply of the record sheets on which the

spaces for the actual cards had been filled in with these five digits, these lists having been carefully prepared for each session from logarithm tables so as to present a truly random sequence. The agent and the first experimenter worked in the inner room of the studio. The agent's chair was always on the side of the card table which was farthest away from the subject's room.

After the agent and the first experimenter were seated and ready, the second experimenter would seat the subject in the anteroom of the studio, always out of sight of the pair in the next room. During an experimental session, the subject never entered the room where the agent worked. The door connecting the two rooms was left open a few inches to permit the first experimenter to give the signal for the start of each run and to keep the subject in step with the agent by calling the serial number of each trial as it occurred. The position of the subject was always such that he could not have seen the agent's table even if the door had been wide open.

After the agent and the first experimenter were seated with the screen between them, the record sheets with the prepared random numbers were taken from a briefcase in which they had remained from the time they had been prepared by Mr. Soal. They were handed to the first experimenter, who looked at the first of the random digits on one of the record sheets and then picked up the correspondingly numbered card and held it at the window in the screen for the agent to see. The agent immediately lifted the picture card in the corresponding position in the box, holding it up just long enough to see what it was and then letting it fall back into its place. At the same time, the experimenter controlling the agent called out, "One." Upon hearing the signal, the subject recorded his impression of the agent's thought. The initial letter of the name of the response was written down from .2 to .4 of a second after the experimenter's signal. The second experimenter always watched the subject to see that he kept strictly in time with the two in the adjoining room. The first experimenter then picked up the numbered card corresponding to the second random digit, the agent glanced at the picture card in that position, the experimenter called out, "Two," and the subject recorded his second call. The procedure was continued through the first run, and then, after a pause of from thirty seconds to a minute, was repeated for the second run

of the record sheet. At the end of the two runs the first experimenter came around the screen to the agent's side of the card table, immediately turned up the five picture cards, and carefully recorded their order in the one-to-five positions at the bottom of the record sheet. The recording of this code was witnessed by the agent and frequently by an observer as well.

B.S.'s calls were finally translated into corresponding numbers and recorded in the column next to the random digits on the first experimenter's record sheet. Then the scoring was done. Sometimes the scores were checked at the end of a few runs, and sometimes only at the end of the entire session of eight to twenty runs. After a few sessions, the subject was never present when the scores were checked, and he was told only in general terms whether the results were good or bad. Both experimenters actively shared in the checking, and the process was witnessed by the agent and the observer. The utmost care was exercised throughout in the handling of the records to avoid all possibility of error.

Before starting a new record sheet, the agent always reshuffled the five picture cards out of sight of the experimenter and replaced them in the box. The experimenter was already seated behind the screen across the card table, and it was impossible that she should have known at any time during the run the order of the picture cards in the agent's box. The agent never spoke at all during the time a test was in progress. There seems to be no possibility that sensory cues were a factor in the subject's success.

At the end of each session a complete copy of the records of the evening was made by the two experimenters and the observer. Immediately after leaving the studio, a member of the group, in the presence of the others, mailed this to Professor C. D. Broad. This second copy of the data serves as a guarantee that none of the records have been lost or changed, and we are informed that Professor Broad has now placed his set of the results on file with the Society for Psychical Research.

Results and Interpretations

The Question of Chance

As in B.S.'s previous results, the most striking scores resulted from hits on some other card than the one upon which the agent

was concentrating when the call was made. With certain exceptions to be described, the general rule was that the subject scored his greatest number of successes on the next succeeding card which the agent was to use. The authors dispose of the question of chance by giving the results for all the experimental "trials" which are involved when the calls are scored against the card to follow. These are called (+1) precognitive trials, and there are, of course, twenty-four such trials in a run when the subject does not leave any blank spaces in his call column of twenty-five spaces. Including all work under all conditions from January 24, 1941, to June 1, 1943, the dates covered by the tests in the report, there was a total of 11,378 (+1) precognitive trials. We are told that absolutely all tests are included, telepathic and clairvoyant alike. "The number of (+1) successes on this grand total is 2,890 compared with an expectation of 2,308.17 by Stevens' method.[3] We have thus an excess of hits on the (+1) cards amounting to 581.83 and equivalent to 13.6 Standard Deviations, with odds of more than 10^{35} to 1 against chance" (p. 36). Odds of this proportion, of course, completely dispose of the hypothesis that the subject's successes represent nothing more than what might be expected by chance.

The points of major interest in the experiments hinge upon the manner in which the scoring was affected by changes in conditions which were effective for a limited number of runs. In discussing the results obtained under different experimental conditions, the authors speak of any result which has a critical ratio of around 3.00 as significant. I shall accept their standard of significance in this discussion of their findings, and I shall not, without special reason, attempt to give the exact statistics upon which the various conclusions and considerations are based.

[3] This is a method of calculating the expected number of hits which takes into account the frequency of occurrence of the five symbols in the two sequences compared: the agent's cards and the subject's calls. The authors recognize that the use of this method leads to an underestimation of the true significance of the scores because of the fact that if ESP is present there would naturally be a tendency for the subject to call more frequently those pictures which occur most frequently in the agent's sequences. The fact that the investigators prefer to use this method nevertheless, accepting the penalty entailed, shows how far they are ready to go to meet every conceivable criticism. Their use of Stevens' method does not affect the interpretation of their results. But I am by no means certain that this is an innovation which should be generally adopted in future experiments of this character.—J. G. P.

Precognition ?

The most interesting question is this: Is precognition really demonstrated? I have taken the liberty of putting quotation marks around the term "precognitive telepathy" in the title of this review to show that there is a problem of interpretation involved here, a problem of which the investigators are fully aware. If in their discussion they seem to prefer the precognition hypothesis to the possible alternative hypothesis of a combination of clairvoyance and telepathy, they make it clear that any hypothesis must be held tentatively, pending further evidence. What are the facts as they weigh in the balance for and against precognition?

In the 1936 work with B.S., in which the agent looked at a card from a pack of twenty-five, no particular strain was put upon the clairvoyance hypothesis to suppose that in the (+1) precognitive successes the subject was simply responding to the card which still remained on top of the deck instead of to the one at which the agent was looking. In the series of tests reported here, however, *there was no deck of cards*, and it is necessary to suppose that, to score by clairvoyance alone or by a combination of telepathy and clairvoyance, the subject would have had to perceive the digit for the next trial on the first experimenter's record sheet, then use clairvoyance to perceive the corresponding card in the agent's box. Even this roundabout approach *via* clairvoyance with a possible admixture of telepathy would probably be preferred by many readers to any explanation involving precognition. But there are further facts which are relevant.

In order to eliminate the possibility of clairvoyance by dispensing with the prepared lists of random numbers, the experimenter controlling the agent was provided with a bowl containing two hundred colored counters, forty of each of five colors. The experimenter selected one of these by touch without looking at them and held it before the window in the screen. The agent then looked at the picture card which was in the position corresponding to the colored counter chosen. During these experiments an observer sat beside the agent and recorded the sequence of colored counters (actually the numbers arbitrarily associated with them) as they appeared at the opening in the screen. The (+1) scores were just as good with this procedure as they had been with the prepared lists of numbers;

and it is not possible to suppose that ESP-of-the-present alone was operating—not, at least, if the counter for the succeeding trial had not yet been chosen.

Inasmuch, however, as the experimenter who selected the counters used both hands, alternating between them, there is a possibility that the counter for the next trial had already been grasped by the hand which was stirring in the bowl at the time that the subject was making his call. If the next counter was ever chosen before, or simultaneously with, a call, it would of course have been possible for the subject to respond to it by clairvoyance just as he might have been responding to the next number on the prepared lists. The authors do not discuss this possibility; but if they did, they would be able to cite other results to which this hypothesis could not be applied. For instance, they deliberately varied the rate of calling, slowing down some runs to intervals of five seconds between trials and speeding up others to about 1.5 seconds. The average normal rate was just under three seconds per trial. In the runs done at the more rapid rate, the subject scored at the chance level on the (+1) cards, but he got significant scores on the second card ahead, those in the (+2) precognitive position. This result came as a surprise when the experiment was first tried; thereafter it occurred consistently with all rapid-tempo runs. Now in these rapid runs it is evident that clairvoyance could not account for the successes with the counters, for the subject was naming cards which were to be chosen by the agent to agree with counters which could not possibly have been selected. That is, the hand of the experimenter which was holding the counter up to the screen at the time of the call would have to return that counter to the bowl and come up with another one before the selection of the card which the subject had already named successfully would be determined!

The investigators also tried direct clairvoyance experiments in which the agent simply touched the picture cards in the box without looking at their faces. These were tried first without the subject's knowledge; later they were alternated with regular telepathic runs with his knowledge. Invariably, the clairvoyance experiments yielded chance results.

Was it possible that the subject was somehow influencing the experimenter's selection of the colored objects, leading her to pick up

those colors which would make the cards agree with the cards he had written down in the preceding trials? In recognition of this possibility, the experiment was varied to see if the subject could influence the experimenter's choice of counters when he deliberately tried to do so. The results are chance, but of course it is entirely possible that, if there had been such a reverse effect in the regular experiment, it would not have shown up under the very different psychological situation involved in deliberately testing for it.

The clairvoyance hypothesis, as an explanation of the (+1) and (+2) results, therefore clearly encounters some real difficulties in the foregoing facts. The precognition hypothesis is direct and equally applicable to all the conditions. Also, after the first few sessions, the subject, hearing the experimenters discuss the precognitive results in the light of that hypothesis, probably came to think of his response as one of prediction. This does not explain why the subject showed this unusual displacement of hits in the first place, and I cannot, of course, be sure that there was any change in his attitude which affected the scoring. There is already strong evidence for the occurrence of precognition from previous studies. There need not, therefore, be exercised so much reserve in applying this hypothesis to the present results as would be the case if this evidence stood entirely alone. I think, as the investigators do, that we must leave the final answer for future work to determine, but I agree that what the subject, B.S., was doing looks most like precognition, and that to a very remarkable degree!

Other Findings

Under this heading will be described very briefly the results of several other variations which were introduced into the experimental conditions.

It is interesting to observe at this point that B.S. did score significantly upon the actual card at which the agent was looking in some of the sessions early in the series. At the end of the first experiment Mr. Soal told B.S. to keep reminding himself during the coming week that he was going to score upon the actual target card. At the next session the scores on the actual target were highly significant, with the odds of one hundred million to one against chance. In fact, in three of the first five sessions, with Miss R.E. serving as

agent, B.S. scored significantly on the actual target card. Likewise, in three of these sessions he scored significantly on the (+1) cards. The third session gave significant scores on both the actual targets and the (+1) cards. The authors write of the subsequent period as follows: "During the succeeding weeks, in which there was an influx of fresh visitors, the interest of everyone became centered on the precognitive scores. We talked to B.S. only of 'precognition.' All this may have had the effect of directing his faculty into the precognitive channel from which it scarcely ever strayed for a whole year" (p. 48). As I have already suggested, it seems not unlikely that the precognitive scoring was partly the result of the directed effort of the subject.

It was found that the subject succeeded in scoring significantly when, instead of the picture cards, the agent looked at cards upon which the initial letters of the animals were printed. Similarly, the results were significant when the words *mane*, *trunk*, *neck*, *beak*, and *stripes* were used to represent the associated animal names of *lion*, *elephant*, *giraffe*, *pelican*, and *zebra*. The substitution of the associated words did not succeed on the first occasion of their introduction when the change was made *entirely without the knowledge of the subject*, but these words were used successfully later when the subject knew that some kind of change in the cards was made with alternate record sheets but did not know what kind of change was involved. The authors sum up by saying, "We think it very probable that *any* symbols which the Agent interpreted as meaning Lion, Elephant, etc., would serve just as well" (p. 73).

It has already been mentioned in another connection that the rate of guessing was varied experimentally. The slower rate gave only chance results. The higher rate caused the hits to be further displaced to the (+2) position, but the degree of success was comparable to that upon the (+1) trials at the normal rate.

The subject succeeded with only three chief agents, Miss R.E., Mrs. G.A., and Mr. J. Al. With eight other agents who participated from time to time to an extent totaling forty-eight runs, B.S. obtained only chance results. Mr. J. Al. had been the chief agent in 1936, and with him B.S. had scored equally well in the same runs on the (+1) precognitive and the (−1) postcognitive trials. When this agent appeared on the scene again, this phenomenon of double

displacement, forward and backward at one time, reappeared. This was the only agent in the present series with whom the spread of successes in both temporal directions occurred. *And when the rate of guessing was approximately doubled, the successes with agent J. Al. occurred with the (+2) and the (−2) trials, skipping right over both the (+1) and (−1) trials!* The phenomenon appeared to be consistent, at least as regards the differences shown among the different agents!

An interesting variation was introduced into the situation on one occasion when the agents R.E. and Mrs. G.A. were both present. Mrs. G.A. was serving as agent while R.E. was present only as an observer. Then, without the knowledge of the subject, R.E. was also brought into the experiment to see if she could influence B.S. to respond to her thoughts of the animal pictures. For the purpose of this experiment, each agent, in each run, used a different arrangement of the five animal cards and each was unaware of the order of the other's cards. The prepared random numbers were used on this occasion, and R.E. sat just behind Mrs. G.A. so that both agents could see the numbers which the experimenter presented at the window. The result was that B.S. continued to score with the agent he *thought* he was working with—Mrs. G.A.—but obtained only chance results with R.E. This result constitutes an interesting beginning upon the general problem of "telepathic intrusion."

Dr. C. E. M. Joad, who was the agent on one occasion for two runs, is listed among those who were successful with the subject, and the circumstances under which he acted in this role are particularly interesting. A footnote to the records for the session of November 21, 1941, reads: "Just before sheet 3 began, Dr. Joad went into the studio and, unknown to (P) [the subject] and (EP) [the second experimenter] in the anteroom, asked R.E. (A) [the agent] to leave her seat while he himself sat in it, reshuffled the cards, and acted as Agent during this sheet. R.E. later reported that she had moved out of sight of the cards while Dr. Joad acted as Agent" (p. 112). B.S. had been scoring well with Miss R.E. as agent on the (+1) cards, and he continued to score above chance expectancy on these cards when the agent was changed without his knowledge. The two runs during which Dr. Joad was "slipped in" as agent

gave (+1) scores of seven and eight. It seems a pity that further trials of this kind were not made on subsequent occasions as the opportunity presented itself.

In fact, the whole question of the role of the different agents is one that is not treated as fully as its importance merits. Only one paragraph of discussion is devoted to this problem. This I quote in full: "It may be asked: 'Do the three good Agents possess any common mental characteristic?' We think that two of them (R.E. and J. Al.) display great passivity of temperament. Both are very intelligent and at the same time very quiet. B.S., on the other hand, is a nervous type, assertive, and a good talker, and has the quick temperament associated with artists. But whether this contrast in psychological type between Agent and Percipient [subject] is important, we do not know. The contrast in type between B.S. and Mrs. G.A. is not so marked" (p. 64). It is to be hoped that this general line of inquiry can be carried much beyond the point reached here.

At the suggestion of Mr. C. A. Mace a nonrandom arrangement of the pictures was tried as an experiment. Without the subject's knowledge, on three different occasions, a run was inserted in which the agent repeated the same card for the first twelve trials of the run and then used another picture exclusively for the next thirteen trials. The subject's (+1) scores for these three runs (of twenty-four trials each, of course) were seven, twelve, and thirteen. These results are significant when evaluated by the CR method using Stevens' formula for obtaining expectation (CR = 2.7) and also when the number of times the subject called the "right" symbols here is compared with his frequency of calling these same symbols in his work as a whole. Here at last is the "stacked deck" experiment which so many people have requested!

Another variation was introduced by having the agent look at the picture cards for the twenty-five trials of the first half of the record sheet and then, for the second run of the sheet, simply touch the backs of the cards, without picking them up, while concentrating upon them from memory. The "seeing" trials and the "memory" trials were both significant, and there was not an appreciable difference between them. Here is further evidence that the subject was truly using telepathy, as these experiments approach the conditions

of the "pure telepathy" experiments of the earlier Duke University work.

The last two experiments in this long series were arranged to be conducted in the rooms of the Society for Psychical Research in order to satisfy anyone who might otherwise choose to believe that the subject, working in his own quarters, "did it with mirrors." Each of these sessions yielded highly significant results.

A record was kept of 396 individual trials upon which the subject felt more confident of success than usual. *He did not do appreciably better* in these than in his work in general. Also, notes were made on various occasions of the fact that the subject felt exceptionally well or that he was ill. The results appeared to have no relation to these reports of physical and psychological conditions. It may be noted that the subject had been discharged from the army because of duodenal trouble and the loss of one kidney.

Part Two of the report comprises a complete list, for each of the forty sessions, of the scores for (0) direct hits, for (+1) and (+2) precognitive hits, and for (−1) and (−2) postcognitive hits, together with a partial cross-check between the two runs on each record sheet. Part Three consists of a chronicle of each session, in which the exact procedure for each occasion is given in great detail. Because of the shortage of paper brought about by the war, however, the minutes of only three sittings are published, and the statement is made that anyone may see the remainder of the chronicle at the rooms of the S.P.R., or it will be sent upon request.

There are a number of appendices which present additional findings based upon fairly technical mathematical analyses. Even in this section, however, the nonmathematical reader will be repaid by examining such parts of the topics discussed as are available to him. I will mention here only that topic which strikes me as being of the greatest importance and which is presented in Appendix B under the title "Effects of Multiple-Determination." Here the authors give additional evidence of a very conclusive nature for an effect which Soal observed with both Mrs. S. and B.S. in his earlier report. This is the effect of reinforcement of the tendency to make a correct call, when the subject is making both postcognitive and precognitive displacements, because the (+1) and (−1) or (+2) and (−2) cards happen to be the same. The authors show clearly for these instances

in which reinforcement is possible that there occurs a significant excess of hits which can be attributed only to the reinforcing effect which the two identical symbols have upon one another. There seems little doubt that this discovery will, as the mystery of this higher order of scoring is unraveled through future research, lead to important advances in our knowledge of ESP and of the mind in general.

Soal and Goldney were successful in introducing as witnesses of the experiments a number of outstanding people who were total strangers to B.S. As far as I am able to judge, these witnesses made no effort to stay in the background but were assigned such conspicuous parts in the tests as that of watching B.S. closely while he wrote his responses to make certain that he did not get out of step with the experimenter's signals from the next room. And still B.S. scored! What a contrast between these results and those of the other subjects reported in the literature, both in England and America, whose scoring dropped abruptly to chance as soon as a stranger was brought in to watch the tests! So far, B.S. is surely a subject apart. But perhaps the discovery of others with an equally remarkable gift will be rendered more likely as a result of the example of his ESP performance.

Surely it is evident that my reaction to the study under review is overwhelmingly favorable, so I trust that I will not be misunderstood if this discussion ends on a slightly critical note. For one thing, I get the feeling that Soal and Goldney do not fully realize just how unique B.S. was in his ESP performance. They do not mention, for example, the study by Russell[4] in which the outstanding performances of subjects in America were analyzed for a displacement effect without the discovery of any evidence of the B.S. type of performance. Nor is the explanation of the failure to find displacement in previous experiments due to the fact (as Soal has suggested in correspondence) that the rate of guessing is so much slower than that used by B.S.; for in some of the experiments analyzed by Russell, the guessing took place at a rate which was at least as fast as that reported in the present case. Whether displacement will appear with other subjects in the future can, of course, only be left

[4] Russell, William. Examination of ESP records for displacement effects. *J. Parapsychol.*, 1943. **7**, 104-17.

for the events themselves to reveal. It is a mystery why Soal's only two successful subjects, Mrs. S. and B.S., showed this temporal displacement. It would, however, be unfortunate if Goldney and Soal were unintentionally to leave the impression with their readers that this effect has most likely been occurring all along in card tests and that they were simply the first to discover and study it.

The report suffers somewhat from the appearance of having grown up first about the separate experimental sessions and of having then been pieced together without ever becoming the unified and integrated whole that could have been desired. On the whole, it makes difficult reading except for a relatively few close students of the literature; but there are interspersed here and there sections which give digests of the most important findings and which serve to encourage the general reader to keep up the struggle through those pages in which the going is tougher.

It seems to me that the writers have quite correctly appraised their own work when they say: "Our experiments, it may be feared, will raise problems rather than solve them. The problem of their interpretation bristles with difficulties" (p. 29). Yes, but I, for one, would like to have seen more effort made toward interpretation and discussion in spite of the difficulties. In the main, the report gives only the experimental facts and results and leaves these to speak for themselves. The discussions are largely confined to sections following the various tables in which the results of separate experiments are presented. It is to be hoped that the investigators will be able to find an early opportunity to go more deeply into the possible psychological significance of their work and that others will find in these results a challenge to indulge in some hard, bold thinking and will take them as the clues to new lines of fruitful research.

THE PK EFFECT: SPECIAL EVIDENCE FROM HIT PATTERNS

I. QUARTER DISTRIBUTIONS OF THE PAGE

By J. B. RHINE and BETTY M. HUMPHREY

ABSTRACT: This is an account, not of a single experiment, but of a comparison of the results of many—all of them experiments on the PK hypothesis conducted to discover whether the mind can directly influence the fall of dice. The records of all PK tests were examined, as far as they were found suitable to the purpose, to ascertain the distribution of hits on the record page. The record page for each series to be analyzed was divided into quarters, and the rate of scoring for each quarter was found. It was discovered that there was a remarkable consistency of patterning of hits in these quarter distributions, or QD's, as the analysis proceeded from series to series until the eighteen available series were covered. In the typical QD pattern the upper left quarter scored the highest and the lower right the lowest, with the other two quarters ranging between. These QD's were evaluated statistically in a number of ways and were found to be, as a group, very significant; that is, not to be accounted for by chance. The odds against the occurrence of such results by chance alone are of the order of a hundred million to one.

The finding of these reliable pattern effects in the QD's of the page extending over so large a group of series (though not in every one of them) gives the strongest impression yet found in parapsychology of order in the working of a causal principle—in this case the PK effect. No other hypothesis among those considered—biased dice, skilled throwing, recording errors, selection or loss of records—can account for the consistent patterning of hit distributions. An extraphysical explanation of the PK effect appears in the light of these results to be required.—Ed.

I. INTRODUCTION

THE general topic dealt with here is the PK hypothesis—the hypothesis that the human mind can directly exert a measurable effect on a material system in the environment. This is no longer a new problem. Many reports of researches on the PK hypothesis have already been published, and others are awaiting publication. The experiments described in these reports were designed to discover whether subjects could influence rolling dice so as to produce sig-

nificant extrachance scores. The cumulative weight of these many experiments has furnished a considerable stock of evidence in favor of the PK hypothesis, evidence which does not reasonably lend itself to any other explanation.

The special study to be reported in this article is not concerned with the original design nor the main results of the PK experiments. It deals with a particular analysis of the experimental records which was not even contemplated by the experimenters at the time the original tests were conducted, an analysis made for *position effects*, or the patterning of hits due to position of the trial in the sequence of the test. The discovery of a characteristic pattern of scoring running through the results of the different researches might serve to identify the hypothetical PK effect and to distinguish it from all other possible causes. Such a characteristic would have to be ascribable to mental, and not explainable by physical, determinants, and would have to be of sufficient magnitude or consistency in its distribution over the various experiments that it could not be attributed to chance. As will be seen, the study consisted of the analysis of record sheets for patterns of hit distribution attributable to the position of trials on the page.

Background of the Search for Position Effects

Declines of success in the run or record column were reported in a number of instances in the earlier ESP investigations by Richet, Jephson, and Estabrooks, and they continued to be reported in later ones. Other position effects, either decline curves or U-curves, were reported by a number of other experimenters—Rhine, Gibson, Pegram, Sharp and Clark, and Martin and Stribic—prior to 1942 when the analyses on which this report is based were begun.[1] The salience studies reported in 1941 and 1942 by the present authors are further examples of position effects in the field of ESP research.

In the initial search for patterns of hit distribution in the records of the PK tests, the first step did not deal with the distribution of hits *in the column*, but rather with the *sequence* of columns. When the early PK tests were done, it was noted that rate of scoring fell off in the course of a few columns. This led to the use of short

[1] Since 1942, decline studies have been reported by Woodruff and Rhine (**45**), Humphrey (**12**), and Taves, Dale, and Murphy (**42, 43**).

sequences of three runs each in order to minimize the depression of scoring. When, many years after the original work, the data were checked for the hit distribution in this three-run sequence, it was found that a very significant decline in score averages had occurred. The first run gave a much higher average than the second and third (38).

The discovery of this three-run decline was followed by a more general study of the left-to-right distribution in sequences of runs on the page, and it was found that a *general horizontal decline* prevailed in the majority of the series of experiments that were analyzed. This distribution was not limited merely to a short sequence of runs, for when the record page as a whole was divided into halves, the left half usually showed a significantly higher average score than did the right.

The vertical distribution, or the pattern of hits in the column on the record page, likewise showed a general trend of decline from top to bottom as it had done in many of the ESP series.

The Quarter Distribution (QD)

Since the analyses made on many of the PK series had disclosed a predominance of declines in both the horizontal and the vertical distributions, it was obviously desirable to combine the two analyses so as to include both effects in a single resultant pattern. This meant, of course, making a quarter distribution study to determine the frequency of hits for the four quarters of the record page. In doing this, we were led to expect a "diagonal" decline—that is, a decline from the upper left to the lower right quarter of the page—as a necessary consequence of the preponderant horizontal and vertical declines throughout the various series. It does not *necessarily* follow, however, that there will be such a diagonal decline *in a given series* if either a horizontal or a vertical decline *alone* prevails.[2] Since it was urgent to learn how widespread and consistent this de-

[2] As a matter of fact, vertical and horizontal distribution studies individually merit separate presentation, especially in view of the fact that many research series are not properly subject to both vertical and horizontal analyses; that is, there are series which are analyzable for vertical distributions, but which cannot be subjected to a horizontal analysis because of the form of the record sheets, and *vice versa*. But subsequent reports will summarize and present these exceptional cases. Beyond question, the more inclusive quarter analysis, combining both distributions, should be given priority.

cline actually was in the individual series, a survey of all available researches was undertaken. The presentation of the results and evaluations of these quarter-page analyses constitutes the purpose of this paper.

Acknowledgment

We are greatly indebted to a considerable number of assistants, mostly Duke students, who, under the supervision of B.M.H., have aided in the checking, arithmetical work, and other aspects of the extensive analyses reported in this paper. The cooperative spirit of the various experimenters who have allowed us to use their data in making the analyses is also appreciated. The work of each one is identified in Table 1. This study has been made possible by financial aid granted partly by the Duke University Research Council and partly by a number of anonymous contributors to the William McDougall Research Fund at Duke University.

II. The Scope of the Study, and the Methods and Results of the QD Analysis

The Criteria Used in the Selection of Records for the QD Analysis

If the QD analyses were to have a general interpretation, it was recognized that the study should be all-inclusive. The only exclusion of material had to be on the basis of failure to meet certain essential criteria. This is not to say that an individual series could not stand alone; an individual research of this character has quite the same statistical independence that any other investigation possesses. Our objective, however, was specifically one of ascertaining how reliable and extensive any discoverable pattern effects were. Therefore, inclusiveness was important.

It was, of course, necessary to have homogeneous conditions throughout the page if the comparison was to be meaningful. The standard of homogeneity required that the entire page be composed of readings from the same dice thrown in the same manner by the same subjects. It was necessary, too, that all the work recorded on the page be from the same experimental session, carried out in unbroken continuity. Even if a subject paused only to allow another subject to take his turn or if he broke up the test recorded on the page into two or more sessions during the same day, the record was

excluded from the analysis because the objective was to have essentially identical experimental conditions for all quarters of the page. Since we are interested in the subject's mental reactions to the page as a structural unit, it was necessary to select for the analysis only those series in which the page record was filled out as an unbroken unit.

The standard of homogeneity also required either that the same target face be used throughout the page or that it be so varied that the hit distribution could not be attributed to the favoring of any particular face.

In some series suitable for the QD analysis, there were certain pages in which some of these changes of condition were introduced in the course of the tests recorded on the page. To exclude the entire page might have introduced improper selection. For example, if the change in condition was made as a consequence of poor scoring, as it may well have been, we would be excluding a selected group of the data. It was regarded as the safest statistical procedure, therefore, to include in the analysis all the runs on the "mixed" record pages up to the point where the first change of condition occurred. Likewise, all incomplete pages were included.[3] This makes the number of trials represented in the four quarters unequal, but the comparison of the quarters is made in any case on the basis of average number of hits per standard run.

The Scope of the QD Study

The QD analysis, then, covers *all the known records* of PK experiments in which the record pages can be subjected to checking for both horizontal and vertical distributions. All pages are included if homogeneous or if the heterogeneity is so evenly distributed as to offer no basis for bias. Likewise, all fragments up to the point of stopping or transition to another condition are included in the study.

Wherever it is possible to pool the data into larger legitimate groupings or series, this is done. It is appropriate only where the conditions and procedure are essentially the same. The larger the group, the more likely it is that any inherent tendency to hit patterning will emerge. Since the most conservative evaluative method

[3] The question of the *effect* of including these fragments is answered in the section entitled "Discussion." The results given there show that the conclusions of the paper do not depend on the inclusion of these incomplete pages.

for the QD study is based upon the *grand pooling* of all the QD's, it will be seen at once that this manner of grouping individual series will make no difference in the final value. They all add up to the same total in any case.

The data subjected to the QD analysis were all obtained from dice-throwing experiments. A wide variety of conditions prevailed. In some experiments, the subject threw the dice from his hand; in others, from a cup; and in still others, the dice were thrown mechanically or semi-mechanically. In all of them, the subject was instructed to attempt to will or influence the dice volitionally by direct mental action, causing them, as far as possible, to turn up with the specified target face or combination of faces. (It is understood, of course, that the target face or faces are always determined before any given test is begun.)

The number of dice thrown at a single time varied extremely in the different experiments, and the size of the dice used varied usually from series to series. Sometimes a number of different kinds of dice were used.

In some series, the record page was made up of the entries of the individual die-readings, but if more than two dice were thrown at a time, the recording of the actual faces was tedious and slowed up the experiment. It was customary, in such a situation, to count the number of hits made for the target face and simply enter the hit score on the record. Thus a page might represent a few or a great many actual die-throws.

Also, most of the individual series represent independent studies, and the types of record sheet used show considerable variation from series to series. For the purpose of the analysis reported here, these variations are not important provided the above-mentioned standards of homogeneity are met.[4] The type of record sheet most commonly used was the standard ESP record blank, an example of which is re-

[4] The only question regarding these variations in record page which might call for further explanation is that of what constitutes a "page." In six of the series analyzed the record sheet was, for convenience, used for two blocks of entries, each block independent of the other. The criterion by which these two blocks of data on the same sheet are regarded as distinct pages is that changes of condition were introduced between the two units. Each of the two blocks of data was evidently regarded by both subject and experimenter as a natural unit and therefore constitutes a "page" within the meaning of this study. For the convenience of the reader who may wish to consult the original reports of these series, they may be identified as items 3, 6, 12, 13, 14, and 18 in Table 1.

produced below in Figure 1. The peculiarities of the record pages of the different series are, of course, described in the original reports. They particularly involve the natural subdivisions of the page called "sets," which will enter prominently into succeeding reports on the QD's of the set and half-set.

Although these various conditions prevailed in the work as a whole, each series offers in itself relatively little variation in conditions, and the record page, as has already been stated, presents in its different quarters a homogeneous set of data. Comparisons there-

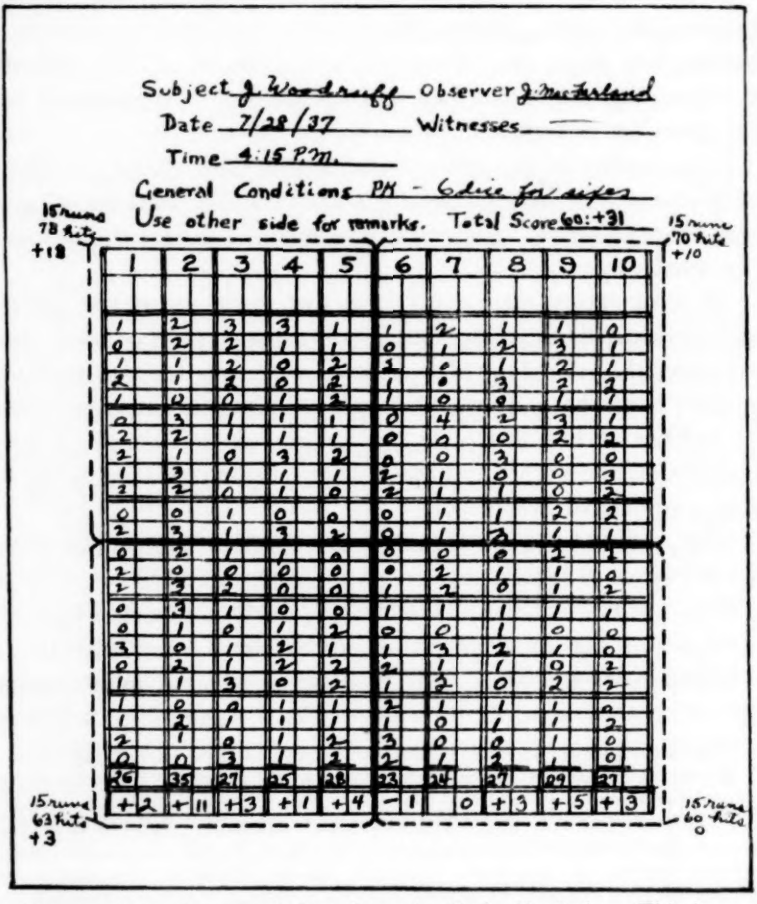


FIG. 1—Copy of a record sheet from the Six-by-Six Series. This is selected as the most typical form and illustrates the quartering of the page for the QD analysis.

fore are possible, and any differences found are to be attributed, not to variation in conditions, but to the subject's reaction either to the structure of the record page or to the test sequence which it represents.

Description of the Analysis and Statistical Measures

The analysis itself was a simple matter. The first step was to quarter the record page by drawing horizontal and vertical median lines (see Figure 1). When the hits for the four quarters were counted, the total was checked against the previously double-checked total of hits for the page. When the QD for the entire series was completed, it was again checked against the totals for the horizontal and vertical distributions as well as against the score total of the series. This system of counter-checking obviously added greatly to the assurance of accuracy in the analyses, for in each series there were thus four independent checks. When any discrepancy occurred, even of a single unit, it was traced down as a matter of rule in spite of the extensive work involved.

The observed number of hits per quarter was divided by the number of runs in the quarter (the standard run of 24 single die-throws was used) to get the average run score per quarter. These average scores are comparable regardless of differences in the number of trials represented in the different quarters.

As we have already explained, the vertical and horizontal declines led us to expect a diagonal decline; that is, a drop from the score average in the first quarter (upper left) to that of the fourth (lower right). This diagonal decline is the focal point of interest in this study. It will be in the measure of the decline shown by the diagonal distribution that the significance of a QD will be judged. The diagonal declines will also be the basis for all comparisons; it is therefore important to measure them in a statistically adequate manner.

There are different ways of evaluating the diagonal distribution. One consists simply in observing in what percentage of cases the QD shows a diagonal decline. Similarly, we may observe the relative size of the score averages in the different quarters and note in what proportion the first quarter is largest and the fourth is lowest of the four.

The most effective evaluation of the QD, however, calls for the statistical measure of the difference between the first and fourth quarters (called the 1-4 difference) against a theory of chance. Throughout this paper we use for this evaluation the CR-of-the-difference method based on the supposition of a binomial distribution. The difference in score averages is divided by the SD of the difference, and the probability is found for the resulting CR by reference to the normal probability integral tables. This is a sufficiently close approximation for our purposes.

There exists, however, the possibility that, because of faulty dice or other factors, the variance may be such that the binomial-CR method is not sufficiently appropriate. To cover this possibility, Dr. J. A. Greenwood suggested the use of what he has called the arc sine method for the CR of the difference (9). This method utilizes a hypothetical variance which is independent of such conditions as faulty dice. It gives a CR which always errs on the side of conservatism. This method has been applied to all the series used for the QD analyses. The CR's obtained by the better-known binomial-CR-of-the-difference technique *so closely approximate* those given by the arc sine method that, because of its greater familiarity to the reader, the former method is used in the text and tables of the main body of this report. However, the CR's of the difference obtained by the arc sine method are given for comparison with the binomial CR's in the Appendix.

The aim of the statistical evaluation of the diagonal distribution is, of course, to see whether the QD pattern shows an extrachance difference in the distribution. While each series has a separate QD analysis and a CR of the difference between the first and fourth quarters, it is the pooled and combined values that are of greatest importance. There are two such general values arrived at. The first and more conservative is obtained by *pooling* the QD's of all the analyzed series and computing the CR of the difference in the diagonal distribution for this pooled QD. The other method consists in combining the individual CR's of the difference for the diagonal distributions by means of the chi-square method and obtaining the probability of this total chi-square for the entire range of series used in the QD study. It is recognized that the latter method preserves the uniqueness of each series and prevents any canceling which

might come from the interplay of opposing trends. The results of both methods will be presented. The second, or chi-square, method is, of course, the only one which permits the combination of series in which a different probability of success per trial obtains. For example, in series in which a single face is the target, the probability of success is one-sixth, while in high-dice tests it is five-twelfths. The results of these different types of test cannot be merged in the form of QD's, and the chi-square method of combination of the CR's of the difference is therefore necessary.

This report has necessarily omitted a great deal of the detailed description of the experiments and analyses. For the most part, the original reports of the individual series contain accounts, not only of the experiments and results, but also of the various distribution analyses for position effects as well. The reader who wishes to go beyond the amount of detail given here may refer to these original reports. In those instances in which the original report is still awaiting publication, we will gladly cooperate with interested readers to provide further information.

Results of the QD Analysis

In considering the results of the QD analysis, it is advantageous to have in mind certain general data regarding the different experimental series on which this study is based. These data have been assembled in Table 1 below and appear in two general sections. Section A, the Singles Section, consists of 12 series. The target in all the tests was a single face of the die. Section B, the Combinations Section, includes six series in which the dice were thrown for a combination of faces—high dice (combinations totaling eight or above), low dice (combinations totaling six or below), sevens, and specific doubles.

The table contains, among other information, the total number of runs originally reported for each series and the total deviation (number of hits above mean chance expectation). These figures are italicized. In the three columns to the right of these data are given the number of runs, the deviation, and the CR of that portion of the records used in the QD analysis of the page. The difference between these two totals of runs represents that part of the original records that could not, according to our criteria, be subjected to the QD analysis.

Table 1

General Data on Series Used in Analysis of the QD of the Page

A. Singles Section

No.	Year	Experimenter	Name of Series	Ref. No. *	Where Done	No. of Subjects	No. Dice Per Throw	Target Face	How Thrown	Total Runs	Total Dev.	Runs Used in QD of Page: Runs	Deviation	CR	χ^2
1	1937	H. L. Frick	Sixty-Dice "Control"	34	Detroit	1	60	6	Cup	*2,172*	*+ 576*	2,172	+ 576	6.77	45.83
2	1936–37	J.B.R., J.L.W., M.P.R.	Six-by-Six	32	Duke Univ.	19	6	6	Cup	*7,776*	*+2,541*	7,440	+2,371	15.06	226.80
3	1934–35	H. L. Frick	Twelve-Medium-Dice	35	Wayne Univ.	2	6,12	1-6	Cup	*1,395*	*+ 204*	1,332	+ 195	2.93	8.58
4	1937	E. P. Gibson	Gibson Machine	8	Grand Rapids	15	3	1-6	Machine	*1,914*	*+ 219*	1,914	+ 219	2.74	7.51
5	1936	J. B. Rhine, J. L. Woodruff, M. M. Price, A. J. Linzmayer	Duke Machine	....	Duke Univ.	11	2	6,3	Machine	*777*	*+ 366*	757	+ 365	7.27	52.85
			(Cup-Machine)	25	Duke Univ.	(3)	(2)	(6)	(Cup-Mach)	*(110)*	*(+ 64)*				
			(Red-White)	13	Duke Univ.	(7)	(2)	(6,3)	(Machine)	*(369)*	*(+ 154)*				
			(Social Factor)	22	Duke Univ.	(1)	(2)	(6)	(Machine)	*(140)*	*(+ 45)*				
			(Small-Medium)	31	Duke Univ.	(7)	(2)	(6)	(Machine)	*(158)*	*(+ 103)*				
6	1934	F. Smith	F. Smith Two-Dice	29	New Haven	2	2	6,4,1,2	Hand	*399*	*+ 130*	368	+ 123	3.51	12.32
7	1935	F. Smith	F. Smith Six-Dice	29	New Haven	1	6	6,4	Hand	*230*	*+ 71*	203	+ 64	2.46	6.05
8	1935–36	H. L. Frick	One-Die Machine	16	Detroit	1	1	6,1	Machine	*240*	*+ 23*	240	+ 23	.81	.66
9	1935–37	E.P.G., C. E. Stuart	Gibson Cup	7	Grand Rapids	1	6	1-6	Cup	*9,333*	*+2,152*	5,130	+1,255	9.60	92.16
10	1943	W. Gatling	Around-the-Die	5	Duke Univ.	8	6	1-6	Cup	*1,242*	*+ 641*	1,242	+ 641	9.97	99.40
11	1937	H. L. Frick	Sixty-Dice	34	Detroit	1	60	6	Cup	*2,172*	*+ 582*	2,172	+ 582	6.84	46.79
12	1934–35	H. L. Frick	Twelve-Small-Dice	35	Wayne Univ.	2	6,12	1-6	Cup	*1,395*	*+ 120*	1,332	+ 114	1.71	2.92
		Total								*29,045*	*+7,625*	24,302	+6,528	22.95	$\Sigma\chi^2$=601.87

B. Combinations Section

13	1934	M. P. Reeves	M.P.R. High-Dice	23	Guilford	1	2	High-Dice	Hand	*492*	*+ 290*	356	+ 221	6.85	46.92
14	1934	M. P. Reeves	M.P.R. Low-Dice	23	Guilford	1	2	Low-Dice	Hand	*435*	*+ 89*	340	+ 74	2.35	5.52
15	1934	Hilton, Baer	Hilton and Baer High-Dice	10	Duke Univ.	2	2	High-Dice	Hand-Mech.	*484*	*+ 130*	484	+ 130	3.46	11.97
16	1934	Hilton	Hilton High-Dice	11	New York	3	2	High-Dice	Hand-Cup	*824*	*+ 243*	824	+ 243	4.95	24.50
17	1934–36	J. B. R., J. L. W.	Sevens	26	Duke Univ.	5	2	Sevens	Machine	*116*	*+ 22*	116	+ 22	1.58	2.50
18	1934	M. P. Reeves	M. P. R. Doubles	24	Guilford	1	2	6,1	Hand	*733*	*+ 38.67*	712	+ 38.67	2.54	6.45
		Total								*3,084*		2,832			$\Sigma\chi^2$=97.86
		Grand Total													$\Sigma\chi^2$=699.73

*Since the names of many of these series do not appear in the titles of their reports, we list here the numbers by which they can be identified in the references at the end of this paper.

The grand totals in the table are of minor interest in this paper. Even the astonishingly large CR's are unrelated to our present inquiry. They may be of secondary interest to the individual reader who has not seen the original reports and who may later on be interested in the relationship between the QD and the total CR.

In passing, it may be observed, however, that a truly large collection of data is represented. In both sections, there are altogether 27,134 runs, and this number represents 651,216 die-throws. The odds against the chance occurrence of such a combination of CR's as large as those given in the table are over 10^{115} to 1. Whatever questions may arise, therefore, the collection of data from which the study has been made cannot be ascribed to chance. But we are concerned in the pages ahead with the results of analyses which are presumably *completely independent* of the CR's insofar as the statistical measures are concerned.

From this point on, it will be of advantage to discuss the two sections, Singles and Combinations, separately, at least in presenting the QD results.

The QD Analyses of the Singles Section. The 12 series of the Singles Section were each given separate QD analysis and the results are stated in Table 2 below in the form of squares representing the quartered pages; that is, each quarter of each square represents the relevant data for the corresponding quarter-page of the records of the series as a whole. There is given in these quarter spaces, first, the number of runs represented in the quarter. (The run is always standard, consisting of 24 single die-readings no matter how many dice were thrown at a time. For example, in the series in which one die was thrown at a time, the run was represented by 24 entries on the record sheet; where 12 dice were thrown at a time, each run represented only two entries.) Next there is given the total deviation for the group of runs represented by that quarter. This is the actual number of hits above or below mean chance expectation and is marked with the + or − sign accordingly. Finally, the average run score for the quarter is given. For the Singles Section, the run score expectation is 4.00. Under each square is given the CR of the difference between the average run scores for the first and fourth quarters; that is, the upper left and lower right. The larger the number of trials represented in the records of the series, the more

*Since the names of many of these series do not appear in the titles of their reports, we list here the numbers by which they can be identified in the references at the end of this paper.

Table 2

QD's of the Individual Singles Series and of Their Pooled Total

1. Sixty-Dice "Control"

630 +241 4.38	630 +140 4.22
570 +168 4.29	342 +27 4.08

$CR_{d(1-4)} = 2.48$

2. Six-by-Six

2286 +833 4.36	1434 +433 4.30
2286 +796 4.35	1434 +309 4.22

$CR_{d(1-4)} = 2.42$

3. Twelve-Medium-Dice

342 +96 4.28	324 +69 4.21
342 +38 4.11	324 − 8 3.98

$CR_{d(1-4)} = 2.12$

4. Gibson Machine

507 +112 4.22	450 +64 4.14
507 +49 4.10	450 − 6 3.99

$CR_{d(1-4)} = 2.12$

5. Duke Machine

228.5 +158 4.69	150 +69 4.46
228.5 +93 4.41	150 +45 4.30

$CR_{d(1-4)} = 2.04$

6. F. Smith Two-Dice

97 +56 4.58	87 +19 4.22
97 +44 4.45	87 + 4 4.05

$CR_{d(1-4)} = 1.97$

7. F. Smith Six-Dice

63.5 +32 4.50	38 +10 4.26
63.5 +28 4.44	38 − 6 3.84

$CR_{d(1-4)} = 1.78$

8. One-Die Machine

60 +17 4.28	60 + 9 4.15
60 0 4.00	60 − 3 3.95

$CR_{d(1-4)} = 1.00$

9. Gibson Cup

1500 +505 4.34	1462.5 +263 4.18
1095 +281 4.26	1072.5 +206 4.19

$CR_{d(1-4)} = 1.98$

10. Around-the-Die

310.5 +194 4.62	310.5 +206 4.66
310.5 +148 4.48	310.5 +93 4.30

$CR_{d(1-4)} = 2.22$

11. Sixty-Dice

642 +243 4.38	600 +163 4.27
540 +83 4.15	390 +93 4.24

$CR_{d(1-4)} = 1.20$

12. Twelve-Small-Dice

342 −25 3.93	324 +38 4.12
342 +42 4.12	324 +59 4.18

$CR_{d(1-4)} = (-)1.77$

Table 2 (Continued)

Singles Total

	Left	Right
Top	7008.5 +2462 4.3513 1	5870 +1483 4.2526 3
Bottom	6441.5 +1770 4.2748 2	4982 +813 4.1632 4

$CR_{d(1-4)} = 5.56$

significant is a given difference between the first and fourth quarters as shown in the table. For example, the Six-by-Six Series is a large series with a smaller difference between the first and fourth quarters than either of the small F. Smith series, but the CR of the difference for the former is larger than that of either of the Smith series.

The order of the QD's in the table is based on the degree to which they conform to the QD pattern that has come to be regarded as typical (because it prevails most widely); that is, the pattern in which the average score of the first quarter (upper left) is highest and that of the fourth quarter (lower right) is lowest. The first eight series show the typical QD pattern and are arranged in the order of the magnitude of their CR's of the difference between the first and fourth quarters.

The typical QD can best be illustrated by the last square in the table which is made up by the pooling or superimposing of the squares for all 12 series. The difference between the first and fourth quarters for the pooled QD of the page for the Singles Section is *very significant, having a CR of 5.56*. The odds against the occurrence of such a CR by chance are of the order of fifty million to one, which is of course beyond the range of argument. Moreover, the CR is, as stated, the most conservative measure of the QD effect, and it is the CR of the Singles Series only. The CR of 5.56 is basis enough for the rejection of the chance hypothesis, entirely apart from the other findings reported earlier or to be given in the rest of

this paper. No correction or reservation is known that would qualify this statement.

The results which have just been summarized in Table 2 can be judged much more conveniently and effectively in graphical representation. In the following figures, the different quarters of the record page are represented by sections of a three dimensional graph. The four sections show by their height the relative deviations of the average run scores; that is, the rate of success of the different quarters of the record page. As an introductory example, we present first in Figure 2 the QD of *the pooled Singles Series*. This is the same composite QD that appears at the end of Table 2. In Figure 2 and in all the graphs of QD's in this paper, the first quarter, or upper left, is presented at the back of the figure. A slight clockwise rotation of the page is made in order to simplify graphical presentation. Note that the second quarter (lower left) and the third quarter (upper right) are almost equal and are almost midway between the first and fourth. This suggests perhaps what the "perfect" or typical QD of the page seems to be.

In a similar way, all the individual QD's of the Singles Section are presented in Figure 3. There it will be evident that the typical QD pattern is represented in eight of the 12 QD's; that is, the first quarter is highest and the fourth lowest in eight of the 12. In only one, the Twelve-Small-Dice Series, is the first lowest and the fourth highest. Since by chance alone one in 12 would be expected to show the typical pattern, a total of eight QD patterns of the type shown in Figure 2 is therefore a significantly large proportion. This is very obvious without the aid of the mathematics of probability. On the binomial theory the expectation of such a result is one in a hundred thousand times.

In 11 out of the 12 series of QD's of the page there is a drop from the first to the fourth quarter. The chance expectation here is approximately one in two. This, too, is a significantly large proportion of cases. The probability of such a result is .00317 according to the binomial hypothesis.

The consistency of the diagonal decline in the QD patterns is therefore in itself a significant effect. This evidence, together with the significant CR of the 1-4 difference of the pooled QD's, affords a double check against the hypothesis of chance. Thus, both

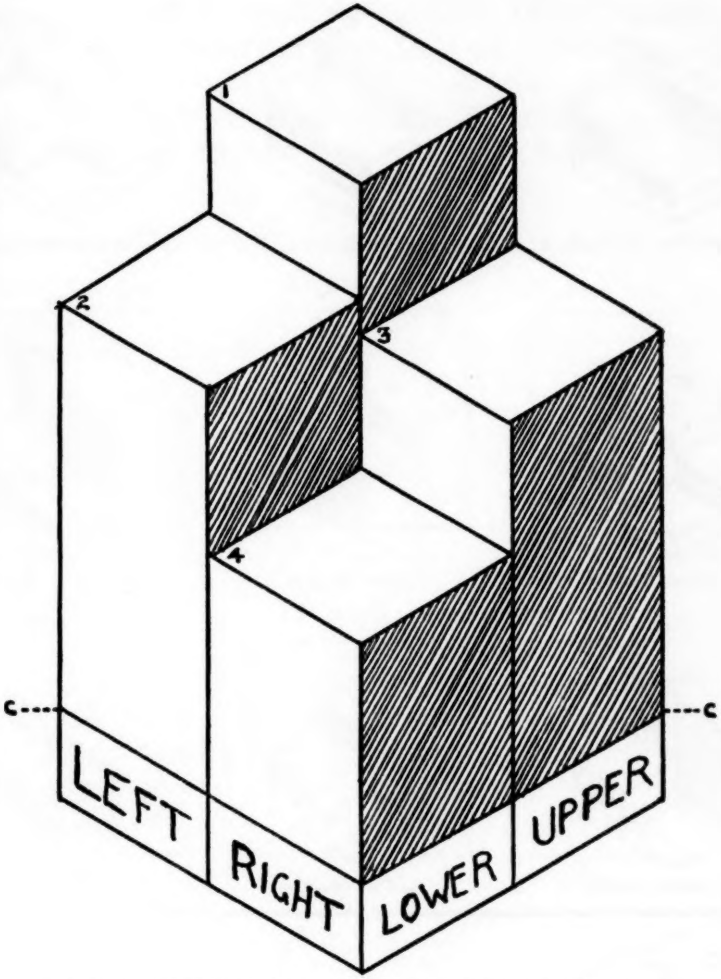
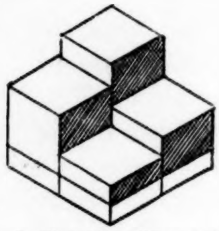


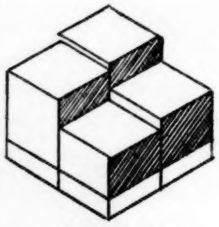
FIG. 2—The QD of the page for the pooled Singles Series in terms of the deviation of the average score for each quarter. The base line (marked "c") indicates chance expectation.

in consistency and in total effect, the QD patterning of the Singles Section is extrachance.

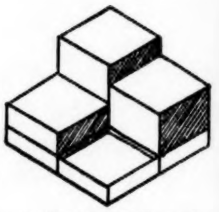
The QD's of the Combinations Section. There are left in Table 1 the six combinations series on which the QD analysis of the page was carried out. These are more or less the odds and ends of the analysis representing a number of different run score expectancies



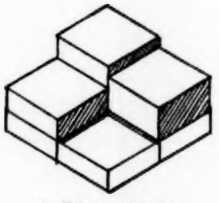
1. Sixty-Dice "Control"



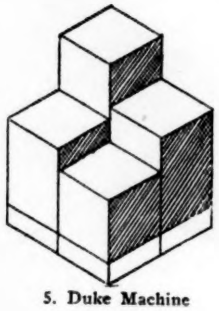
2. Six-by-Six



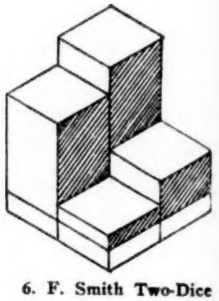
3. Twelve-Medium-Dice



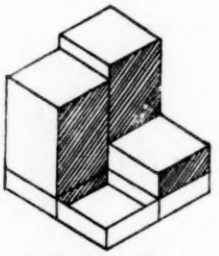
4. Gibson Machine



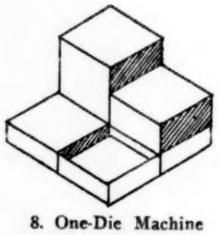
5. Duke Machine



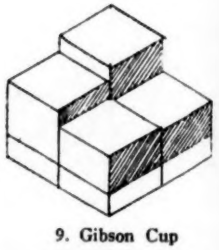
6. F. Smith Two-Dice



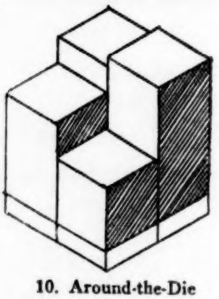
7. F. Smith Six-Dice



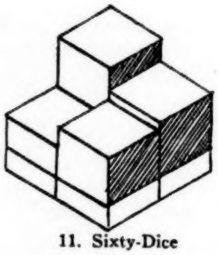
8. One-Die Machine



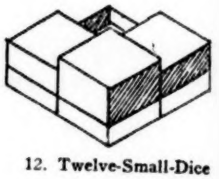
9. Gibson Cup



10. Around-the-Die



11. Sixty-Dice



12. Twelve-Small-Dice

FIG. 3—The QD's of the page for the individual series comprising the Singles Section.

which do not allow a general pooling of the QD's. The QD results of these series are presented in Table 3 in a manner essentially comparable to the procedure in Table 2. At the bottom is given *a pooled QD* for the four High-Dice and Low-Dice Series. The other two QD's are for the Sevens and M.P.R. Doubles Series.

Table 3

QD's of the Page for the Combinations Series*

M.P.R. High-Dice

99.5 +101.5 6.02	78.5 +35.5 5.45
99.5 +66.5 5.67	78.5 +17.5 5.22

$CR_{d(1-4)} = 3.09$

M.P.R. Low-Dice

94 +68 5.72	76 +15 5.20
94 −5 4.95	76 −4 4.95

$CR_{d(1-4)} = 2.94$

Hilton and Baer High-Dice

121 +27 5.22	121 +14 5.12
121 +37 5.31	121 +52 5.43

$CR_{d(1-4)} = (-).94$

Hilton High-Dice

216 +91 5.42	216 +31 5.14
216 +64 5.30	216 +57 5.26

$CR_{d(1-4)} = .96$

Sevens Series

37 +17 2.46	21 0 2.00
37 +7 2.19	21 −2 1.90

$CR_{d(1-4)} = 1.60$

M.P.R. Doubles

178 +17.67 .43	178 +.67 .33
178 +5.67 .36	178 +14.67 .41

$CR_{d(1-4)} = .28$

Total High-Dice and Low-Dice Series

530.5 +287.5 5.54 1	491.5 +95.5 5.19 3
530.5 +162.5 5.31 2	491.5 +122.5 5.25 4

$CR_{d(1-4)} = 2.74$

*Chance expectation for high dice and low dice is 5 hits per run; for sevens, 2 hits per run; for specified doubles, 1 hit in 3 runs, which is equal to .33 hits per run.

The M.P.R. High-Dice Series and the Sevens Series show typical QD patterns. Three of the remaining four series show a decline from the first to the fourth quarters. From the point of view of the general basis of evaluation—the CR of the difference between the first and fourth quarters—we find in this Combinations Section a quite independent confirmation for the QD results of the Singles Section. It will be noted that the CR of the 1-4 difference for the totals of the High- and Low-Dice Series is 2.74. This is a significant value. The major feature, then, of the QD pattern—namely, the *diagonal decline*—is given statistical confirmation in the QD's of the page of the Combinations Section.

The Significance of the Two Sections Combined. Since the Singles Section and the High- and Low-Dice Series have been shown to be individually significant, with CR's of the 1-4 difference of 5.56 and 2.74, respectively, it is not necessary to go further and consider these results in combination. There may, however, be some advantage in having the figures on record. Furthermore, the CR of the difference of 2.74 for the High- and Low-Dice Series does not include the Sevens and Doubles Series.

Since the probability of success per single trial, or the rate of expectancy, is different for the Singles Section tests and for those of the Combinations Section, it is necessary to use the chi-square method of combining the QD results. We present in Table 4 the results of the chi-square combination of the CR's of the 1-4 difference of all series in each of these studies of the record page. In it are given the essential data for the first quarter and the fourth quarter, the difference between the two average scores, the CR's of the difference for individual series, and the chi-square (CR^2) for each series. The chi-squares are totaled for the two sections separately and collectively. The High-Dice and Low-Dice Series are also combined as a group. The corresponding probabilities are given for the different chi-square totals represented.

The grand total of all the chi-squares of the QD's, 69.25, with 18 degrees of freedom, would be expected by chance only once in about one hundred million such collections of records. Such odds are tremendously large and fully confirm the earlier estimates given of the odds against chance for the individual sections. Odds of over one hundred thousand to one are applicable to the chi-square of the

Table 4

THE CHI-SQUARE COMBINATION OF THE CR'S OF THE DIFFERENCES OF ALL SERIES IN THE QD STUDY

A. SINGLES SECTION

No.	Name of Series	1st Quarter			4th Quarter			Av. Score Diff.	CR_d	χ^2
		Runs	Dev.	Av. Score	Runs	Dev.	Av. Score			
1	Sixty-Dice "Control"	630	+ 241	4.38	342	+ 27	4.08	.30	2.48	6.15
2	Six-by-Six	2,286	+ 833	4.36	1,434	+309	4.22	.14	2.42	5.86
3	Twelve-Medium-Dice	342	+ 96	4.28	324	− 8	3.98	.30	2.12	4.49
4	Gibson Machine	507	+ 112	4.22	450	− 6	3.99	.23	2.12	4.49
5	Duke Machine	228.5	+ 158	4.69	150	+ 45	4.30	.39	2.04	4.16
6	F. Smith Two-Dice	97	+ 56	4.58	87	+ 4	4.05	.53	1.97	3.88
7	F. Smith Six-Dice	63.5	+ 32	4.50	38	− 6	3.84	.66	1.78	3.17
8	One-Die Machine	60	+ 17	4.28	60	− 3	3.95	.33	1.00	1.00
9	Gibson Cup	1,500	+ 505	4.34	1,072.5	+206	4.19	.15	1.98	3.92
10	Around-the-Die	310.5	+ 194	4.62	310.5	+ 93	4.30	.32	2.22	4.93
11	Sixty-Dice	642	+ 243	4.38	390	+ 93	4.24	.14	1.20	1.44
12	Twelve-Small-Dice	342	− 25	3.93	324	+ 59	4.18	.25	1.77	3.13
	Total Section A	7,008.5	+2,462	4.35	4,982	+813	4.16	.19	5.56	$\Sigma\chi^2=46.62$ [d.f. =12] [P = <.000,01]

B. COMBINATIONS SECTION

13	M.P.R. High-Dice	99.5	+101.5	6.02	78.5	+ 17.5	5.22	.80	3.09	9.55
14	M.P.R. Low-Dice	94	+ 68	5.72	76	− 4	4.95	.78	2.94	8.64
15	Hilton and Baer High-Dice	121	+ 27	5.22	121	+ 52	5.43	.21	.94	.88
16	Hilton High-Dice	216	+ 91	5.42	216	+ 57	5.26	.16	.96	.92
	Total High- and Low-Dice	530.5	+287.5	5.54	491.5	+122.5	5.25	.29	2.74	$\Sigma\chi^2=19.99$ [d.f. =4] [P = .0005]
17	Sevens	37	+ 17	2.46	21	− 2	1.90	.56	1.60	2.56
18	M.P.R. Doubles	178	+17.67	.43	178	+14.67	.41	.02	.28	.08
	Total Section B	.....	.....	.....	.....	.....	.....	.....	.....	$\Sigma\chi^2=22.63$ [d.f. =6] [P = <.001]
	GRAND TOTAL	.....	.....	.....	.....	.....	.....	.....	.....	$\Sigma\chi^2=69.25$ [d.f. =18] [P = .000,000,01]

Singles Section taken alone. For the Combinations Section it would be approximately one thousand to one, and for the grouping of High-Dice and Low-Dice Series, approximately two thousand to one.

Results in Brief.. The following summary of the main statistical findings will help to recall the scattered evaluative data:

1. The CR of the 1-4 difference of the Singles Section is 5.56.
2. That of the High-Dice and Low-Dice Series is 2.74.
3. The chi-square combination of the Singles Section represents odds of over one hundred thousand to one against the occurrence of such results (as the CR's of the 1-4 difference in the QD) by chance alone.
4. The comparable figures for the Combinations Section are one thousand to one.
5. Those for both Sections combined are about one hundred million to one.

III. Discussion

Counter-Hypotheses

It is necessary as a first step in the interpretation of the results to consider whether the evidence establishes the PK hypothesis which the experiments were designed to test and which the analyses were to retest. In the evaluation of the QD analyses and their significance for the PK hypothesis, the various outstanding alternative hypotheses are to be considered as a matter of course.

The principal alternative hypothesis—that the results are chance —has been dealt with throughout the section on "Results" and will need but little further discussion. Among other questions that need to be considered, the first that commonly comes to mind is this: Could possible physical defects in the structure of the dice have produced the results under discussion? Again, we must ask whether it is possible to explain the results by supposing that the various subjects influenced the dice by manual skill. Likewise, there is the counter-hypothesis that errors in observation and recording may have been the explanation. Also, it may be worth considering the possibility that the loss or improper selection of records could have resulted in the data which are reported. Finally, if we followed this vein far enough, ethical considerations would eventually come up for question, and possibly even the sanity of the observers.

Each of the original reports of the series used in the QD analysis contains its own evidence and argument against these hypotheses. The discussion here, however, is something in itself, going beyond the treatment of any individual series. It is the peculiar virtue of the QD evidence that it disposes of these hypotheses *en masse;* it is no "island by island" approach. The case for PK is transferred out of range of these counter-hypotheses by the results of the QD study.

The Chance Hypothesis. To begin with, the series used in the analysis were not composed of chance data. The significant CR's of Table 1 indicate this. Furthermore, the CR's of the 1-4 difference of the QD's, as shown in Tables 2 and 3, constitute especially strong evidence against the chance hypothesis. The CR's of the pooled QD's for the two tables, 5.56 and 2.74, afford incontestable evidence against chance as an explanation. The odds against the occurrence of two such CR's by chance alone are of the order of over 1,000,000 to 1. Chance is not the explanation.

Hypothesis of Faulty Dice. The QD results cannot be accounted for by any known kind of physical imperfection in the dice. The recurrent patterning of hit frequency requires a fluctuating principle of causation. None of the known dice defects could be supposed to vary periodically during the filling out of the record sheet. Temperature, moisture, or other physical changes resulting from handling the dice would not vary with the record page. But if they did, or even if there were some temporary loading of the dice at the beginning of each page which caused a decline as it wore off, there would remain the fact that in some series the *dice were handled entirely mechanically* (they were rotated in electrically driven, wire mesh cages) and there was no opportunity for the application of any temporary treatment (even if this were possible in any of the other experiments). The Gibson Machine Series and the Duke Machine Series combined give a QD whose $CR_{d\ (1\text{-}4)}$ is quite significant.[5] This QD is shown below.

Finally, it may be questioned whether some defect in the dice might not exist which would vary in its effect on the results as the

[5] In the One-Die Machine Series the die was handled between trials; therefore this series is not included. In the other two machine series the machine ran continuously throughout the run. There was no stopping after each trial since the rate of rotation allowed ample time for the reading of the dice.

POOLED QD OF THE PAGE FOR SERIES WITH MACHINE-THROWN DICE

2,671 Runs

735.5 +270 4.367	600 +133 4.197
735.5 +142 4.218	600 + 39 4.065

$CR_{d(1-4)} = 3.01$

subject altered his manner of throwing. This manner of throwing might be supposed to change periodically with progress over the record page. The answer to this supposition would be precisely the same as that in the supposed case of the variable defect just discussed in the preceding paragraph; that is, mechanical handling of the dice rules out this further variation of the faulty-dice hypothesis.

The Question of Skill in Throwing. There are, as is well known, persons who can demonstrate considerable skill in the handling of dice, *provided they are allowed to specify certain conditions in the performance.* In this provision, however, lies the essential difference between the sleight-of-hand and the experimental demonstration. We need only to refer again to the significant evidence of the QD analysis of the results of the machine series to show that skill in throwing cannot explain the results. *Skills of all kinds* are completely removed from the experimental situation by the use of the *electrically driven cages.* Here the argument against the skill hypothesis might well rest.

An independent case, however, can also be made against this hypothesis of skilled throwing on the basis of the cup-thrown series taken alone. These series make up a total of 20,856 runs, and their pooled QD gives a CR of the difference between the first and fourth quarters of 4.33. This significant CR is ample evidence against chance, and the use of the cup was, under the conditions, a strong guarantee against the use of skills or tricks in the throwing of the dice.

POOLED QD FOR ALL CUP-THROWN SERIES

20,856 Runs

6066.5 +2099 4.346	5089 +1319 4.259
5499.5 +1550 4.282	4201 + 785 4.187

$CR_{d(1-4)}=4.33$

The Question of Errors in Recording. Let us suppose that enough errors favoring the PK hypothesis were made in the recording to have produced the entire deviation of 6,528 hits for the Singles Section alone (shown in Table 1); we would still be at a loss to explain why the errors distributed themselves over the record page in the manner which produced the diagonal decline. The recorders did not know—no one knew—that years afterward the QD analysis would be made. *How could the many various recorders independently fall into the same type of distribution* of their errors to so significant an extent? Recording errors could not be the explanation.

The Question of Favorable Selection of Records. The question we put to ourselves here is this: Could the group of CR's upon which the significance of this work is based be the result of improper selection of data? Could there be other data inadvertently lost or deliberately excluded which properly belong in this grouping of series and which, if included, might appreciably alter the statistical findings? The answer is simple. *All* the PK researches with which we are acquainted have been subjected to the QD analysis as far as that was possible without violation of the specific requirements laid down; and, since the PK research originated here and reports on it have not been published until recently, it seems very likely that no other considerable body of data could have been assembled in PK experiments without having come to our attention. Because of the existence of summaries and totals made by the respective experi-

menters at the completion of the original experiments (with the exception of one minor series), it would not be possible to have lost part of these records without discovery. But even if these facts were not as stated, the extremely significant probability of one in a hundred million for the total chi-square of the QD's in Table 4 is such that *many times the number of series reported here could have been lost, hidden, or forgotten without rendering insignificant the final probability of the whole.*

This leaves only the question of what the principles of selection were which determined whether a given series was legitimately usable in the QD analysis. These principles were explained in the section on "Criteria." The essential point is that the record page had to be homogeneous so as to make the data of the quarters truly comparable. What was wanted was a similar situation throughout the area in which the comparisons were to be made; namely, the record page.

In a few series the subject varied the target face as a matter of routine; perhaps every run on the page would have a different target face, or else the target face for a given set would be changed for the next set on the page. Now if the variation of these different target faces for the different quarters of the page was sufficient to avoid the excessive use of a particular target face in a given quarter of the page, such series were included in the QD analysis. In the individual reports concerned, the grounds for this inclusion are discussed in full, and the reader is able to judge for himself the propriety of the analysis.

The best immediate answer to the question of the propriety of including the series with varied targets, however, is that the evidence of the QD analysis on which the conclusions of this paper rest need not at all depend upon any of these series. With all of them excluded, the pooled QD of the Singles Section alone still shows a CR of the 1-4 difference of 4.39, and this, in view of the reduced number of runs represented, indicates that there is no essential difference even in the rate of decline represented.[6] The

[6] As an extreme reassurance on this question, it may be of interest to know that even if we had included the series composed of heterogeneous page records (discussed later), there would still be the highly significant CR of the 1-4 difference of 3.80 given by the pooled page QD of the Singles Series alone. The significant CR of the difference for the pooled High- and Low-Dice Series (2.74) would remain unchanged.

fragmentary record pages, which, as we explained in the section on "Criteria," were included (by a rule made in advance) in order to avoid danger of favorable selection, do not seriously affect the total results. Their inclusion actually raises the average 1-4 difference of the pooled Singles Series from .1709 to .1881. But this alteration is less than a standard deviation of the difference and is negligible. Without the incomplete pages the pooled QD of the Singles Section has a CR of the 1-4 difference of 4.54.

Accuracy and Repeatability of the Analyses

While the QD analyses have been made with considerable care and are based upon methods of rechecking and safeguarding that have been well proved through years of analysis, ***we would still welcome further checking and analysis by any interested investigators.*** If there are those whose reluctance to admit the validity of the PK effect is such that they will attribute the apparently invincible proof contained in the QD analysis to errors in procedure, we would point out that these proofs are based upon objective records that can be examined and re-examined until there is no basis of disagreement left among those who are interested or concerned.[7]

The QD analysis is in all essential respects the equivalent of a *repeatable experiment*. If persons other than those who conducted the analysis can obtain findings similar to ours by following the same rules and procedures and going over the same series, the requirements of independent confirmation are met. These analyses of the PK research data thus enable us to take a position comparable to that of the geologist who of course does not deal in *repeatable experiments* but rather in *repeatable observations*. If we are right, it would seem reasonable to say that this is something of a turning-point in parapsychological methodology.

IV. Special Findings

Thus far we have expounded a new and powerful proof of the validity of PK. This is the main thesis of the present paper. We shall attempt now, however, to describe some findings from the QD analyses that indicate how PK works in the test situation represented. In the discussion of these results, we shall proceed on the

[7] For a report of an independent reanalysis of the major portion of the series used in this QD study, see Dr. J. G. Pratt's paper in this issue of the JOURNAL.

understanding that the PK hypothesis is established as the best explanation of the results covered in this study.

Internal Consistency of the QD

Some impression of the extent of the typical QD pattern among the records available for analysis has of course been given in the report of results. Further knowledge concerning its extent is of interest and is important to the judgment of the nature of the factors producing the patterning. It will, for example, be desirable to know more about the degree to which this hit patterning persists in the further subdivision of the record page. Analyses have been made of the QD's of the page subdivisions which are, for convenience, called "sets" and "half-sets." The QD's of these record blocks occurring within the page contribute in themselves findings of importance equal to those of the QD of the page. In the interest of convenience in presentation we will make separate reports of these QD's of the set and half-set.

There are other questions to be answered concerning the extent of the QD patterning. For instance: Are the QD patterns found to be comparable within sections of the work of the same subject? There are two Frank Smith series in the group on which the QD's of the page have been determined. The similarity between the QD's given by these two series constitutes a feature of interest. The figures are given below in Table 5 and a graphical representation is furnished in Figure 4. The important thing to note here is that Smith himself was the subject in both series.

There is likewise some resemblance between the results of the QD analysis of the two Reeves series, the M.P.R. Low-Dice Series and the M.P.R. High-Dice Series with M.P.R. as subject. This is true in spite of the fact that different target combinations were used for the two. The high-dice recording was done at the top of the record page, and the low-dice in the lower half. This in itself may have introduced a secondary effect of structuring, though it cannot be determined whether such was the case. Here again, as the table and figure indicate, there is outstanding similarity, especially in the salience of the first quarter.

In two other series by a single subject under reasonably similar sets of conditions, we find QD's which are somewhat alike. We refer

Table 5

COMPARISON OF QD'S OF THE SAME SUBJECTS IN DIFFERENT SERIES (IN TERMS OF THE DEVIATION OF THE AVERAGE RUN SCORE)

Subject: F. Smith

F. Smith Two-Dice		F. Smith Six-Dice	
+.57	+.07	+.50	+.26
+.50	+.11	+.44	−.16

Subject: Mrs. Reeves

M.P.R. High-Dice		M.P.R. Low-Dice	
+1.02	+.45	+.72	+.20
+.67	+.22	−.05	−.04

Subject: H. L. Frick

Sixty-Dice		Sixty-Dice "Control"	
+.38	+.27	+.38	+.22
+.15	+.24	+.29	+.08

Subject: Mrs. Gibson

Gibson Machine		Gibson Cup	
+.55	+.22	+.29	+.07
+.15	−.03	+.14	+.13

to the Sixty-Dice Series and the Sixty-Dice "Control" Series with Frick as the subject in both. These series were intended to have identical conditions except for the target combination: in the Sixty-Dice Series the subject tried for sixes, while in the Sixty-Dice "Control" Series he intended to *avoid* them (although he continued checking for them). Actually, both series gave approximately the same number of sixes. These series will come up for discussion later; it will be sufficient here simply to refer to the QD results shown in the table and figure.

Finally, we list the figures for two series of tests with Gibson's subject, L.H.G. (Mrs. Gibson). In order that the basis of comparison for the two series be approximately the same, we have chosen the first 4,980 entries from the large Gibson Cup Series and the (entire) 5,040 entries of L.H.G.'s work in the Machine Series. The two series were quite different, however, and perhaps the same degree of similarity in QD's should not be expected. In one series the dice were machine-thrown; in the other a cup was used. One was done with three dice per throw, the other with six. In one the

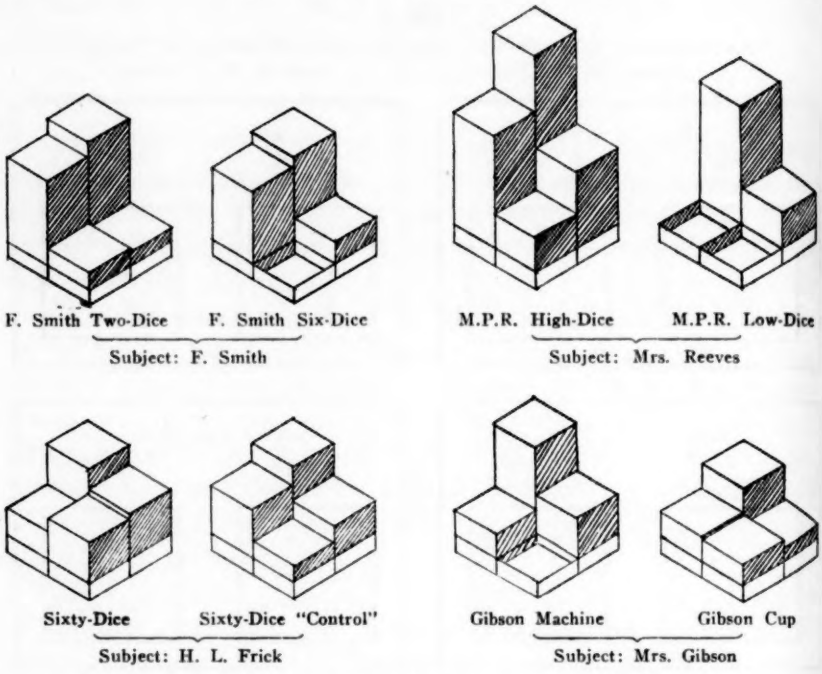


FIG. 4—Comparison of the QD's of the same subjects in different series.

set was three columns of 24 entries each; in the other, ten columns of 6 entries each. We suggest that this difference in set structure is of greatest importance among the various conditions.

It seems probable that when the analysis of quarter distribution can be carried further and the work of a given subject compared in various experimental subdivisions, further similarities will be found.

The Horizontal and Vertical Distributions in the QD

It has doubtless been noted that the QD's have shown somewhat more of a decline in the horizontal direction than in the vertical. This is true not only of the Singles Section but also of the High- and Low-Dice Series of the Combinations Section. The figures are summarized below in Table 6.

It is interesting to see that the horizontal declines in both sections are represented by significant CR's of the difference, while only one of the vertical distributions in the table has a CR of the difference

Table 6

COMPARISON OF HORIZONTAL AND VERTICAL DISTRIBUTIONS OF THE QD'S OF THE PAGE
(IN TERMS OF THE DEVIATION OF THE AVERAGE RUN SCORE)

Section of Results	Vertical				Horizontal			
	Top	Bottom	Diff.	$CR_{diff.}$	Left	Right	Diff.	$CR_{diff.}$
Singles Series.....	+.31	+.23	.08	3.39	+.31	+.21	.10	4.36
High-Dice and Low-Dice.....	+.37	+.28	.09	1.27	+.42	+.22	.20	2.68

above the standard of significance. The other vertical distribution, however, shows a decline and a positive CR of the difference.

The important question arises as to whether there is any relation between horizontal and vertical declines. The data are at present inadequate for judgment.

Special Cases

Some of the most valuable insights often come from a study of the exceptional manifestations of a phenomenon. Our attention is especially drawn to exceptional instances in the PK tests as we attempt to find out more about the conditions which affect performance.

The Sixty-Dice "Control" Series. This "control" series immediately followed the Sixty-Dice Series. In the latter the target was the six-face and 582 hits above expectation were obtained in the 2,172 runs. Then, as a *control,* the Sixty-Dice "Control" Series was carried out, with the same subject, dice, and number of runs and with the same objective conditions. In this series, however, the opposite face, the one-face, was the target, but *sixes alone were recorded. No record of the ones was made.* After a time, failing to reduce sixes below expectation, the subject, H.L.F., resorted to an attitude of merely *opposing sixes,* simply willing throughout the rest of the series that no sixes come up. He obtained 576 sixes in excess of expectation for the 2,172 runs, very nearly the same number of hits obtained in the preceding series. Both series, concluded H.L.F., must therefore have been due to imperfections in the dice favoring the six-face.

From the QD analyses, however, it may be seen (Table 2) that the faulty-dice hypothesis is not at all applicable to the way in which

the hits are distributed over the page. The Sixty-Dice "Control" Series gave a normal QD pattern—one more typical, in fact, than that of the original Sixty-Dice Series! Moreover, this "control" series gave a significant CR of the 1-4 difference (CR = 2.48), while the Sixty-Dice Series gave a CR of the 1-4 difference of only 1.20. The significant result of the "control" series argues equally as well against the chance hypothesis as against the hypothesis of faulty dice.

What, then, are we to suppose, brought about this result when the subject himself was trying to avoid sixes? The one thing that stands out most clearly in the PK research, as in the ESP work, is the fact that *the essential mental act is unconscious.* No one knows *how* he influences the die, or even *when* he is succeeding and *when* he is failing. Now, with an unconscious process at work, it is fairly obvious that it would be hazardous to go on counting sixes as Frick did if the aim were to suppress them. Merely counting them gives the sixes a certain basis of interest; and their superior stimulus value (showing a more striking figure-ground contrast than the other faces) works toward the same effect. Without realizing it, the subject may have continued to try for sixes in much the same degree as in the preceding series.

Twelve-Small-Dice Series. In quite another way the Twelve-Small-Dice Series offers a unique case. This series, it will be recalled, is that in which 12 small dice were thrown along with 12 medium dice; all 24 being thrown from a cup at one time. Both sizes of dice were recorded separately and each size is treated as a separate series. The medium dice have been presented as the Twelve-Medium-Dice Series, which gives a normal QD pattern. (See Table 2 or Figure 3.) The small dice, on the other hand, are unique in that they give a completely opposite distribution, the first quarter being the lowest and the fourth quarter the highest of the four. The fact that these two sizes of dice were thrown together and were therefore subject to the same conditions except for their size and the psychological variables that go with response to size, makes this QD reversal of special interest. The results are not properly attributable to chance. While there were no significant CR's of the difference in either series taken alone, the original report shows that the two series taken together offer adequate statistical assurance that chance is not the explanation. Since the evidence is not

easily condensed, we refer the reader to the original report (35) to be published soon.

Our hypothesis that the explanation of this reversal is a displacement of the PK effect is as yet so inadequately supported experimentally that we do not elaborate it here. But displacement effects are not new. Readers of the ESP literature are already familiar with one form in the hitting of the wrong (but nearby) target, as reported first by W. Carington[8] (2) and later by Soal (41) and Thouless (44). In the PK work itself, displacement from the designated target face or combination of faces and the favoring of another target have already been encountered. The occurrence of sevens to a disproportionate degree in the M.P.R. Low-Dice Series (23) was pointed out, and a similar effect, even more marked, is reported in the Low-Dice Series by J.B.R. (26); another instance in the Hilton, Baer, and Rhine data awaits publication. In all cases there is evidence that the appearance of this displacement effect on the unexpected targets accompanies the wandering-off of the normal PK effect from the main target objective; hence the interest in the simultaneous decline of the medium dice and the incline of the small in the two series whose QD's are under discussion.

Pooled QD of All Series Analyzed, Including Those with Exceptional Conditions

We have omitted from our report four series of experiments in which the conditions did not satisfy the criterion of page homogeneity required for QD purposes as laid down at the outset. These four series are the McDougall (33), the Alternation (17), the Small and Medium Drug (15), and the Six-by-Six Drug Series (36). There is no need, perhaps, to raise the question as to the propriety of leaving these out of the general QD survey; but if the reader is interested in what the effect would have been had these series been included, he is referred to the following table which gives the result of pooling these four irregular QD's with the 12 QD's of the Singles Section of Table 2. The data are of no other interest than that of meeting the question raised.

[8] This was incidentally observed earlier by Bruck (1), Sinclair (40) and perhaps others; but Carington first evaluated the effect statistically.

POOLED QD OF SINGLES SECTION WITH INELIGIBLE SERIES INCLUDED FOR COMPARISON

37,539 Runs

10,840 +2,984 4.275	8,664.5 +1,923 4.220
10,273 +2,511 4.246	7,776.5 +1,335 4.172

$CR_{d(1-4)} = 3.80$

Some Further Comments on Conditions Under Which QD's Occur

One striking range of variation in the conditions is in the number of dice per throw. In the One-Die Machine Series only one die was thrown at a time; whereas, at the other extreme, in the Sixty-Dice "Control" Series, 60 dice were thrown at a time. Various other numbers per throw between these extremes were used in some of the other series, the most common number being two; but 6, 12, and 24 were also thrown at a time. No outstanding difference has been discovered between the series with different numbers of dice per throw. The One-Die Machine Series shows only a small CR of the 1-4 difference, but its distribution is approximately as typical as that of the Sixty-Dice "Control" Series, though much smaller in extent. The average score differences between the first and fourth quarters are nearly the same for both series (.33 and .30).

Another variation of conditions which does not seem to have affected the QD is that of the manner of throwing. Whether the dice were thrown by hand, from a cup, or by machine, the QD gave significant CR's of the 1-4 difference. (The hand-thrown Singles Series gave 2.62; the cup-thrown, 4.33; and the machine, 3.01. Average differences are .57, .16, and .30, respectively.) From one point of view at least, the throwing represents an outstanding variation of the conditions. In one case the subject had manual contact with the dice and engaged in motor behavior in the test; with the machine, however, he had no manual contact whatever with them.

The number of die-throws represented on a single page also ranges broadly. For example, in the One-Die Machine Series, the records of 240 single die-throws make up a page; whereas, in the Sixty-Dice "Control" Series, the completely filled pages have 8,640 single die-throws represented, 36 times as many. The finding of a similar QD pattern for these two extremes affords some further data concerning the nature of the QD effect.

Possibly something illuminating may yet be found by the study of the effect of the particular structure of the set upon the type of page QD pattern. One of the most interesting observations of this nature can be had by comparing the two Gibson series, the Cup and Machine. The Machine Series was recorded in columns of 24 throws, three columns to the set, usually with six sets to the page. The Gibson Cup Series, on the other hand, was recorded in short columns of six entries to the column, 10 columns to the set. From four to six sets, rarely more, made up the page. Now the page QD's of the Gibson Machine Series gave a better pattern and a more significant CR of the 1-4 difference than did the Gibson Cup Series, although the general scoring rate was lower for the Machine Series. It is probable that the long column was advantageous to the position effect though not to the rate of scoring. Incidentally, the same subject (L.H.G.) who was mainly responsible for the QD pattern in the one series was the principal subject in the other.

V. The Significance of the QD Findings

We shall be working at the task of appraising the significance of the QD and other position effects in the PK research for a long time to come. Anything we undertake to point out now is probably of value only for the research immediately ahead rather than for any long-standing interpretation. By long odds, the greatest importance of the QD's right now is their extraordinary evidential value in the establishment of the PK effect itself. There is hardly need for restraint in emphasizing this point. It is perhaps rare in the history of the experimental investigation that so wholly incidental a finding as the QD effects, and one so unanticipated in the original experiments, has assumed such strategic importance in the ultimate evaluation of the work. We have already pointed out

how the QD eliminates the alternatives to the PK hypothesis in the explanation of the experimental results.

There is, however, another contribution of the QD analysis which goes considerably beyond that of the establishment of the PK hypothesis. We refer to the aspect of lawfulness and order which the many recurrences of the typical QD pattern have contributed. The significant CR's of the 1-4 difference in the QD's are totally and individually of great importance, but the repetition of these QD patterns from series to series in the work of different experimenters with different subjects and quite varied conditions so far exceeds anything of the kind that has been encountered in parapsychical researches in the past as to constitute a major development and to mark an epoch in the field of inquiry.

This effect of lawfulness evidenced in the QD analyses of the page records is not, however, confined to the PK researches themselves. The ESP investigations on which suitable analyses have been made, have shown from the beginning some clear-cut evidences of position effects upon the rate of scoring. It is true, these effects of position in the ESP researches have largely been either declines or U-curves. There has not been to our knowledge any extensive analysis of the horizontal distribution of the page in the ESP experiments, but it waits only for time and opportunity. The fact that in many ESP researches there are vertical decline effects comparable to those found in the PK records suggests that it may be worth while to carry out a complete QD analysis of all the available ESP records.

The effect upon performance of the place of the trial in a sequence is not, however, limited to parapsychological studies. Similar position effects have shown up in the investigation of the more familiar processes, such as memory, learning, and sensory perception. The typical U-curve, familiar in studies of recall, is an example; this, too, usually shows a considerable drop in rate of success from the first half to the second half of a column. Gradients or declines resulting from the effect of position upon performance are familiar in the reports of learning experiments. For example, in maze tests with rats, proximity of choice-point to the goal measurably increases the success in learning; in sensory perception, the salience of the end positions is noted. These examples can readily be supplemented from daily life. The first stanza of a poem is more easily recalled than

the second, and the first few names of a list stand out more than those in the middle.

What we are encountering, then, in the position effects in the PK records is but the working of a set of presumably very general principles applied, of course, to a special type of process presented in PK. This discovery of the familiar and general within the particular effect before us helps greatly to "naturalize" the phenomenon of PK, especially for those who find it hard to accept because it is unfamiliar. We do not, it is true, understand the essential function of PK even when we find it patterning its successes in this somewhat familiar fashion represented in the QD's. We must remember that in these much more familiar processes just referred to—memory, learning, and sensory perception—we are still unacquainted with the fundamental processes involved, but we know much about them—as for example, these position effects. Our point is that this "knowing-things-about" a phenomenon constitutes a great deal of our so-called understanding and certainly a great deal of what makes the phenomenon acceptable.

We have left for a final word what seems to us the most significant aspect of the QD study: its bearing upon the fundamental character of the PK function. Our first step toward basic knowledge of a mental process is to discover, if we can, what its relation is to our more familiar realm of knowledge, the physical. It seems to us that the main contribution of the PK research, and in particular of the QD analysis, is its bearing upon that question.

First we wish to interpret the QD results of the Twelve-Small-Dice Series in conjunction with those of the Twelve-Medium-Dice. These two series, it will be recalled, are based upon the throwing of 12 medium dice and 12 small dice from the same cup at one time. Opposite patterns of QD resulted. As we had already stated, these distributions taken together are significant. Now it is evident that some discrimination was shown between these two sizes of dice, mingled though they were in the shaking and throwing from the cup. We cannot rationally conceive of such discrimination's being the effect of a physical force directed *en masse* upon this moving conglomeration of 24 dice. There must have been something of what we call "purposive selection" exerted, something which involves identification and cognitive orientation. Yet all our acquaint-

ance with the physical world to date leaves us with no means of giving a physical explanation of this intelligent action. Were it simply a matter of hitting one size of die and doing nothing to the other, we might think of doing some kind of simple screening activity which would bear upon dice of a certain size and allow others to escape. But the PK effect was plainly exerted upon both sizes of the dice, gradually shifting from one size to the other as progress across the page was made. This variable discrimination seems definitely nonphysical in character.

The Sixty-Dice "Control" Series produced a QD pattern with a more significant CR of the difference and with a more typical scoring distribution than that of the One-Die Machine Series. Recognizedly, the longer the series, other things being equal, the more typical and the more significant should be the QD pattern. Nevertheless, the two series give approximately similar patterns. They represent the widest extreme in conditions appearing in all the reports that have been submitted for analysis, the number of dice at a throw in the one case having been 60 and in the other, one. If a physical principle is at work, it is difficult to conceive of its operating as effectively in the case of the 60 dice-per-throw test as in the case of the one-die test. We may, of course, point also to the better score average of the Sixty-Dice Series, but in this discussion we are confining ourselves to the QD's alone.

The same argument may be applied to the superiority of the Twelve-Medium-Dice tests over the One-Die Machine Series, or of the Sixty-Dice "Control" Series over the Duke Machine Series, in which two dice were used. The point in general which stands out from these comparisons is that so far as the QD pattern is concerned (and so far as the score average is concerned), there is no favoring of results with limitation of the number of dice per throw. Large numbers succeed as well as small. *Where in the physical world is there a true analogy for this?*

There is perhaps a superficial analogy in the mass effects of gravitation which would affect one die or 60 dice with no appreciable difference. Here in the PK tests, however, we are dealing with a focused or *directed* force (for example, one that affects one set of dice and avoids another, as in the 12-dice series) scarcely able to have an effect upon a single die, so limited is its influence. Yet,

while we can only with the greatest care discover its slight effect on one die, we find it affects 60 dice in approximately the same degree and manner. The directed or focused energies that we know of in physics today would not be expected to give such results.

Finally, there is the argument derived from the M.P.R. High- and Low-Dice Series. In both of these we find QD's that are quite typical and that give significant CR's of the 1-4 difference. They contribute two of the most striking diagonal declines in the entire study. These results were obtained from tests in which the subject had not the simple target of a single face, but rather a combination of faces. The target combination of high dice or low dice represents obviously a much more complex goal than that of a single target face. Since two dice must be influenced, a computation is necessary in the course of the throw. How can we conceive of setting an ever-so-complex physical system to work on a pair of dice in such fashion as not only to influence the dice but also to produce intelligently a given combination of faces?

May we not say, moreover, that it is literally inconceivable that any physical force could operate upon *even a single die* in any *intelligent* way? And how other than as "intelligent" can one characterize the operation of a factor which must follow the die in its fall and turn it at the right point of time and space? It is obvious to any observer that even with a single die the subject cannot follow with his sense organs the random course of the falling cube. Yet any force to be applied must take account of position and time. There is no kind of perception possible here except an extrasensory kind of perception. If there are two dice or six or 60, there is presumably increased complication, at least on the analogy of sensory and mechanical effects with which we are familiar. But it is quite as much out of the question for a physical system as it is for a sensory system to follow the dice and influence them intelligently.

Thus the argument against a physical explanation of the PK effect does not rest on the QD analyses alone. We can even say that this evidence is less specifically "anti-physical" than certain other types of PK evidence. The point we do wish to make clear is that the QD evidence, which on general grounds is of superior character, likewise leads to the same conclusion concerning the nonphysicality of this phenomenon.

If this final point is sound, the ultimate consequence of the QD studies will be to further the revolution in our conceptions of the nature of man which the ESP work has already begun. That is to say, if we have found, as appears to us to be true, convincing evidence of the operation of a nonphysical force, originating in the human mind and manifesting itself in mechanical effects in the environment, we foresee as an inevitable outcome a fundamental shift in explanatory theory of human personality—a shift from the present academic trend of concentration upon brain physics as the center of man's individual world toward the conception of an extraphysical mind which dominates personal life.

To realize the significance of this shift, we need only remember that, contrary to the academic view, our social institutions and our working views of man and society assume that man is composed of a nonphysical mind which dominates, at least to a degree, the physical system of his organism and his environment. Freedom of choice, the basic concept of social living as we think of it today, is hardly tenable without such a basis. Before we undertake to discover whether it is possible to surrender this concept of individual freedom and to adjust our social institutions to a completely physical philosophy of personality, it is surely in order to try to discover what the fundamental character of the individual actually is. These researches have led us to believe that we have the channel to such discovery open and that the following-up of these and related studies may lead us eventually to the desired knowledge.

VI. Summary

The 18 series which have been found suitable for analysis for the hit distribution of the quarter page have shown a general diagonal decline effect that is remarkably consistent from series to series. This diagonal decline represents a drop in score average from the upper left quarter of the page to the lower right. The difference between these two quarters is statistically very significant. (A summary of the statistical evaluation is given at the end of the section on "Results" in Part II.)

The 18 series are presented in two general sections—Singles and Combinations—and both sections show diagonal declines with significant differences.

This analytic treatment of the PK results furnishes a superior type of evidence in favor of the PK hypothesis. With the degree of reliability which this diagonal decline is shown to have, the QD study is regarded as a crucial case against the various counter-hypotheses which commonly confront the PK research. None of these counter-explanations could be supposed to produce the QD's and their significant differences.

The occurrence of a typical QD pattern in so large a number of the series available for analysis shows a degree of lawfulness in the operation of the PK effect that is in itself the most impressive aspect of the entire investigation.

The QD results afford further evidence of the nonphysical character of the PK effect.

Appendix

Comparison of the CR's of the 1-4 Difference as Computed by Two Different Methods

A. Singles Section

No.	Series	1st Quarter		4th Quarter		Binomial CR Method: $CR_{d(1-4)}$	Arc Sine Method: $CR_{d(1-4)}$
		Trials*	Hits	Trials	Hits		
1	Sixty-Dice "Control"	15,120	2,761	8,208	1,395	2.48	2.41
2	Six-by-Six	54,864	9,977	34,416	6,045	2.42	2.38
3	Twelve-Medium-Dice	8,208	1,464	7,776	1,288	2.12	2.12
4	Gibson Machine	12,168	2,140	10,800	1,794	2.12	1.97
5	Duke Machine	5,484	1,072	3,600	645	2.04	1.94
6	F. Smith Two-Dice	2,328	444	2,088	352	1.97	1.90
7	F. Smith Six-Dice	1,524	286	912	146	1.78	1.74
8	One-Die Machine	1,440	257	1,440	237	1.00	.99
9	Gibson Cup	36,000	6,505	25,740	4,496	1.98	1.92
10	Around-the-Die	7,452	1,436	7,452	1,335	2.22	2.14
11	Sixty-Dice	15,408	2,811	9,360	1,653	1.20	1.15
12	Twelve-Small-Dice	8,208	1,343	7,776	1,355	(−) 1.77	(−) 1.80
	Total Section A	168,204	30,496	119,568	20,741	5.56	5.40
	B. Combinations Section						
13	M.P.R. High-Dice	1,194	599	942	410	3.09	3.06
14	M.P.R. Low-Dice	1,128	538	912	376	2.94	2.93
15	Hilton and Baer High-Dice	1,452	632	1,452	657	.94	.93
16	Hilton High-Dice	2,592	1,171	2,592	1,137	.96	.95
	Total High- and Low-Dice	6,366	2,940	5,898	2,580	2.74	2.62
17	Sevens	444	91	252	40	1.60	1.53
18	M.P.R. Doubles	2,136	77	2,136	74	.28	.24

*By "Trial" here is meant a single die-throw.

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A REINVESTIGATION OF THE QUARTER DISTRIBUTION OF THE (PK) PAGE

By J. G. Pratt

Early in December, 1943, it became possible for me to accept an invitation to return to the Parapsychology Laboratory at Duke University for a short period. I came for the purpose of making an independent study of the statistical analyses of the quarter distribution reported by Rhine and Humphrey in their article in this number of the Journal. Herewith is a brief account of the results of that recheck.

Two questions were foremost in my mind while I was conducting my investigation: 1. Did the authors select the material for the analyses of the quarter distribution of the page in strict accordance with the criteria which they established and which are described in their report? 2. Were the analyses which were made upon that material free from checking, counting, and computational errors which might affect the conclusions?

1. The question of the selection of records for analysis has three aspects: *a.* which experiments should be included; *b.* which runs, if any, should be excluded for one reason or another in those experiments actually used; and *c.* the ethical question of the deliberate destruction or alteration of the records to favor the QD effect.

a. I have examined all records of PK experiments which I have been able to find in the files of the Parapsychology Laboratory and I have found none, except the eighteen studies reported, in which the records lend themselves to an analysis of hits in the quarters of the page. There are, as the authors point out, some additional records on which it would be physically possible to make a QD analysis (these are the ineligible studies which the authors mention in their paper), but these fall short of the criteria for inclusion which were set up. I have found no evidence that the selection of data for inclusion was based on a knowledge of how the results would affect

the analysis. *b.* I concur fully in all instances in which parts of pages have been omitted from the QD analysis in those studies which were included. The reasons which necessitated omissions, under the criteria, are clearly evident from the original record sheets. Omissions most commonly are made because of a long temporal break in the middle of a page or because of the appearance of the work of an incidental subject in a long series of a single major subject. I am convinced that all such omissions were fully justified on logical grounds and that they were made both without a knowledge of, and without regard to, how they would affect the QD results. *c.* Throughout my investigation I have scrutinized the records with a view to detecting any evidence of deliberate manipulation of the records in a manner that would favor the QD effect. Most of the records are in pencil on soft paper, and it would be an easy matter to discover erasures and changes. In fact, some of these have been made in the M.P.R. High- and Low-Dice Series, but I am informed that they were made by a volunteer student assistant who was assigned the task of circling the hits in these series with a red pencil. It is evident from a study of these few erasures that what happened was that occasionally a "miss" was circled by mistake, the subject's original record was erased to remove the red mark, and the record was then re-written in pencil. These erasures are all such that they occur only on misses and they could not have any relation to the QD effect. The theoretical possibility that unfavorable pages were deleted from some series and completely destroyed is forestalled by the fact that the general summaries for these series (both the number of die-throws and the scores) were known and were on record before the QD analyses were made. Any effort on the part of anyone to destroy unfavorable sections of the records would therefore have been detected eventually.

2. The limited amount of time at my disposal for rechecking made it necessary to decide at the outset what would constitute an adequate confirmation of the statistical analyses. I decided in favor of rechecking completely those series which carry the main weight of the conclusions in the Rhine-Humphrey paper rather than sampling for errors in all the series. To date, I have completely reanalyzed the following studies for the hits in the four quarters of the page and have recomputed the CR's for the 1-4 difference in the

QD: (1) Gibson Machine, (2) Sixty-Dice "Control," (3) Six-by-Six, (4) Duke Machine, (5) F. Smith Six-Dice, (6) Around-the-Die, (7) Sixty-Dice, (8) M.P.R. High-Dice, (9) M.P.R. Low-Dice, and (10) the F. Smith Two-Dice. All of the above studies except the Gibson Machine Series were analyzed originally by Miss Humphrey; and in the work done by her and under her direction, I found not a single error. This statement has special significance for the studies which I did not have time to recheck, as B.M.H. was also responsible for the analyses in all of those. In the Gibson Machine Series, which was analyzed in the absence of B.M.H. by assistants, I found four single hits which had been scored in the wrong quarter (in these instances the error was unfavorable to the conclusions; that is, it was such as to lower the difference between the first and fourth quarters). In addition, I found that in the case of nine runs which properly belonged to the left half of the page, only the hits had been counted in the two left quarters while the runs themselves had been counted on the right-hand side of the page. This run-transposition error had the effect of raising the average score in the first quarter and lowering it in the fourth. The correction lowered the CR of the 1-4 difference by .4, changing it from 2.52 to 2.12.

In view of the fact that I have worked over the series with the most significant CR's of the difference, the recheck is entirely confirmatory. My investigation has demonstrated that the decline between the first and fourth quarters of the page is a highly significant and genuine statistical effect; that is, even if the worst possible assumption is made regarding the eight series which I did not recheck, and even if their CR's are completely discounted (taken as zero), still the 18 series as a whole would be highly significant (chi-square = 52; d.f. = 18; $P = < .000{,}075$), with odds of more than 10,000 to 1 against occurrence by chance.

[The corrections which were necessary as a result of the discovery of the errors in the Gibson Machine Series have all been made in the preceding paper. These errors did not in any way affect the conclusions.—Ed.]

TYRRELL'S STUDY OF APPARITIONS: A REVIEW[1]

By Margaret Pegram Reeves

Tyrrell originally set out with the intention of making a general survey of all the work yielding positive results undertaken by the Society for Psychical Research throughout the sixty years of its existence, but he became so impressed by the significance of the evidence of apparitions that he decided to confine his discussion to that topic.

By way of introduction, he makes a brief survey of the prevailing attitude toward psychical research at the time of the founding of the Society, of the spirit in which the work was carried out, of the reception which it received, and of its current status in the mind of the public:

> Neither in the scientific nor philosophical world, nor in any department of human thought has there been any tendency to approach psychical research in the keen spirit of the explorer. . . . I do not think this attitude has anything to do with the strength or weakness of the evidence. It arises from the fact that the kind of things which psychical research brings to light do not fit in with a very generally accepted world-outlook.

Tyrrell proposes a very broad definition of psychical research: the exploration of human personality; and he regards psychology as a branch of that field. He finds that psychical research is the "key to the Human Situation; all the great problems of existence devolve upon it." This aspect of the importance of the human personality is emphasized by the prevailing chaos in the social situation, based, Tyrrell believes, on the split by political parties founded on diametrically opposed views about the value of personality. He places on psychical researchers the obligation, not only of collecting evidence, but also of clarifying and integrating hypotheses and presenting them in a framework which will "blend with all the established

[1] Tyrrell, G. N. M. *Apparitions.* London: The Society for Psychical Research, 1942.

facts of science and common experience, though not with the commonly accepted philosophy."

Considerable importance is attached to the high incidence of hallucinations as demonstrated by the Census of Hallucinations wherein seventeen thousand persons replied to the question: "Have you ever, when believing yourself to be completely awake, had a vivid impression of seeing or being touched by a living being or inanimate object, or of hearing a voice; which impression, so far as you could discover, was not due to any external physical cause?" There were 1,684 affirmative replies, a proportion of one in ten. Of these, 1,087 were visual hallucinations and 493 were auditory. Of the visual experiences, 283 occurred in the presence of one or more other persons; in 95 of these cases, some other person or persons shared in the percipience. Of the total of 493 auditory cases, 94 occurred when the percipient was not alone, and of these, 34 were collectively experienced. Thus in about one-third of the cases in which the percipient was not alone, some other person or persons in the group shared the experience.

The credibility of the narrators, the strength of the corroborative evidence, and the multiplicity of the cases reported suffice to convince Tyrrell that while no one case may be unquestionably a theoretically perfect one, "cases which reach the standard required for publication by the Society for Psychical Research are genuinely faggot-building cases." Perhaps even more strongly convincing than the external evidence for the veracity of the reports is the less obvious internal evidence for their genuineness, the remarkable consistency of occasion, form, and content of the experiences described. And since this agreement does not fall along the lines indicated by the traditional classical "ghost-story" formula, the indications are that the compatibility is further evidence of the good faith and the accurate reporting of the narrators.

Characteristics of Apparitions

Tyrrell designates four classes of apparitions: (1) *experimental cases,* those in which an agent has deliberately tried to make his apparition visible to a particular percipient; (2) *crisis-cases,* those in which a recognized apparition is seen at or near the time when the person represented by the apparition was undergoing some crisis;

(3) *post-mortem cases*, those in which the recognized apparition appears so long after the death of the person represented that no coincidence can be supposed; (4) *ghosts*, apparitions which habitually haunt certain places. It is realized, however, that these distinctions cannot always be maintained. For instance, unrecognized figures may sometimes be seen only once, or recognized figures of living people may be seen when there is no coinciding crisis in their lives.

The problem of collective experience of apparitions has come in for a great deal of consideration; but Tyrrell rejects both the *invasion theory* of Myers and the *infection theory* of Gurney[2] although he is in accord with their belief that the phenomenon has to do with telepathic influences rather than with the presence of any physical object.

Preparatory to offering his own view, Tyrrell lists the following characteristics evidenced by apparitions: (1) Visual apparitions appear in different kinds of space, varying as to whether they seem to be part of the physical space occupied by the percipient, whether the percipient seems to be transported to a different physical space, whether there are two phenomenally intermingled spaces, etc. (2) None of the classes of apparitions with which Tyrrell deals in the present report are physical phenomena. Although they may at times appear to perform physical acts, these acts are found to have been hallucinatory in the same way that the figure itself is hallucinatory. (3) Apparitions are remarkably like normal perceptions; they behave as if aware of their surroundings, adapting themselves to the physical environment in which they are seen. (4) Apparitions may be equipped with clothing; they may be accompanied by another figure, human or animal; and they may be supplied with inanimate objects such as canes, carriages, or fishing poles. (5) The hallucination may be collectively experienced. While Gurney hesitated to concede that the percipients saw the *same* thing, holding that the experiences might be better described as *similar*, Tyrrell finds that the evidence, carefully looked into, leaves little room for reasonable doubt that what is seen is not merely more or less similar but the same. (6) Not always, but frequently, the percipient experiences

[2] Myers assumed an extraphysical or "metetherial" space and held that the apparition which is jointly perceived must occupy this metetherial world of space, but not *physical* space. Gurney relied upon a supposition of an "infectious" spread of telepathy to explain the multiple hallucination.

a sensation of cold. Sometimes he also experiences other subjective feelings; particularly frequent is what is described as feeling a "presence."

All the characteristics do not, of course, appear in every case: in fact, apparitions give one the impression of being phenomena which attain expression in a fragmentary and imperfect way only. And they show an individual originality and contempt for rule which makes them not easy to classify. Moreover some percipients are better than others, and some seem to be almost incapable of perceiving an apparition at all. There is also a variation in the power of percipience with the same individual from time to time. Nearly all the cases on record are incomplete in one way or another, and the number of cases in which the three senses of sight, hearing, and touch have all been hallucinated together is very small. Compared with the marvellous perfection of normal sense-perception, the perception of apparitions is a very amateur performance indeed. But it is profoundly instructive.

Through this analysis of group-characteristics, Tyrrell derives a "Perfect Apparition" and stands it for consideration beside a normal human being. Several comparisons may be made: (1) Both figures stand out in space and appear equally solid. (2) There is no detectable difference between the two figures when they are viewed from various points of distance and perspective. (3) Both figures are best seen in a good light; they disappear in the dark. (4) Both figures obscure the background. (5) If the apparition happens to be wearing a rose, its smell is perceptible. (6) Characteristic sounds, such as footsteps, breathing, or the rustling of clothes, may accompany the approach of both. (7) Both figures may appear to be aware of their surroundings; if either should touch us, the effect would be the same. (8) Either might speak to us, but we should probably not be able to engage the apparition in lengthy conversation. (9) Both would be reflected in a properly placed mirror. (10) Both figures might cast shadows. (11) Both would disappear and reappear when we closed and reopened our eyes. (12) Both figures might be clothed, carry objects, or be accompanied by other figures. (13) We might seem to see and hear both of them open and close doors or pick up objects. (14) In contrast to our experience with another person, we might feel a sensation of cold in relation to the presence

of an apparition. (15) In an encounter with an apparition we probably would not feel any resistance if we tried to touch the figure, although it is usually impossible to test this because the apparition appears to avoid contact. (16) An apparition would not leave footprints. (17) An apparition probably would not appear in a photograph nor its voice be recorded on an apparatus should such an attempt be made. (18) The apparition would disappear after a time. (19) An apparition might differ from a man by being slightly luminous or transparent, by being able to disappear through walls or to make other unconventional exits, or by displaying details so minute that in normal perception they would not be observable.

This is a picture of what an apparition would be like at its best, according to our collected evidence; and this is what, throughout the ages, has been called a "spirit." But clearly it is in reality a psychological phenomenon, the explanation of which must be sought in the processes of sense-perception.

Before delving into the difficult subject of sense-perception, however, let us note that the "perfect apparition" has an important bearing on the strength of the evidence on which it is based. It was pointed out above that apparitional cases form a natural group—a fact long ago stressed by Gurney. But now, with the "perfect apparition" before us, we can see much more clearly what he meant by this. All the stories combine to describe an apparition having two apparently contradictory characteristics. The Group-characteristics embodied in the "perfect apparition" are those of a collection of some hundreds of narratives sent by independent witnesses. How is it that these independent narratives conspire to describe phenomena having two characteristics which, to common sense, seem to contradict one another? These characteristics are, (i) the apparition imitates the behaviour of a normal, material figure with almost miraculous fidelity, appealing convincingly to the senses of sight, hearing and touch and behaving at the same time as if it were conscious of what it is doing; yet all the while (ii) the apparition proves itself to be a nonphysical phenomenon, or, in ordinary language, it proves that there is "nothing there." Would any ordinary person, drawing on his imagination to make a good story, string together two such apparent contradictions? It is true that these two characteristics are not really contradictory, but it takes a fairly profound expedition into philosophy to show how they are compatible. . . . Anyone who suggests that the spontaneous evidence is invalid must begin by accounting for the presence of these two group-characteristics.

Theory of Apparitions

Tyrrell then makes this "profound expedition into philosophy." He prefaces his discussion of hallucinatory perceptions with a review of normal sensory perception. Summarizing the facts of sensory perception which are pertinent to hallucinatory perception, he states:

> The point to bear in mind is that a material thing is composed of two entirely different constituents, (1) a physical object, which is a region of space characterised by certain causal properties, and which exists independently of the observer, and (2) a group, or groups, of sense-data, which are private to the observer and are originated by him. It is with these sense-data alone that the percipient has any direct acquaintance. The sense-data are related in a complicated way to the physical object, but this part of the subject may, for present purposes, be ignored.
>
> Matter is thus a hybrid. It is not something which is just there waiting to be found. It is something which we, during the process of perception, have had a hand in constructing.

On the other hand:

> An apparition lacks physical, causal properties and cannot affect the physical organs of perception. Yet sense-data exactly similar to those which occur in normal perception arise; and not bare sense-data only—complete acts of visual, auditory and tactual *perception* are built up on the basis of these sense-data. Perception, then, clearly need not be dependent on the operation of physical sense-organs or on any physical processes whatever in the external world. "Hallucinations," which reach the standard of normal percepts in principle, even if they are to some extent imperfect on any given occasion, can be originated by *psychological* factors in the personality.

Not only are apparitions independent of the usual external causes (stimuli), but they are also of sufficient strength to obliterate partially or wholly the effect which would normally be induced by them and which would conflict with the hallucination. Therefore the sense-data which would normally form the background of the apparition are usually inhibited so that the hallucinated figure appears opaque and the objects behind it are invisible. Conflict between these two modes of perception is evidenced by occasional transparency of the hallucinated figure.

The whole complicated nature of perception leads Tyrrell to speculate that the sense-data are not the result of *direct* physical

causation, are not directly dependent on the physical processes of the brain, but that "they may be *psychologically* caused, the physical brain processes in normal perception playing a guiding and conditioning rather than a causative rôle. . . ."

This psychological constituent would have to be considered as operating both in normal perception and in hallucinations as well, but as being set in operation in two different ways: from "below" in normal perception, from "above" where the initiating impulse is an idea.

Tyrrell likens this "psychological manipulator of sense-data" to a producer or a stage carpenter and assigns to it the task of converting both ideas and sensory stimuli into percepts. For assuming that the personality may be described as being composed of various levels or layers, he has ample precedent both among members of the Society and among psychologists and psychoanalysts in general. He seems to draw on Myers and on McDougall for his theory of the structure of personality:

> These intra-personal elements of personality have sometimes been referred to in addresses, as in Lord Balfour's Presidential Address in 1906, and in Professor William McDougall's Presidential Address in 1920. In the latter the human personality is regarded as an organized and graded hierarchy with consciousness at its head. In such a hierarchy the intermediate members lie between the Self on the one hand and the Body on the other and possess characteristics which are intermediate between those of consciousness and those of matter. I am supposing that those mid-level elements which produce and express an apparition possess some such mid-level characteristics as Professor McDougall suggests.

In the case of the telepathic hallucination, the mid-level functions of the agent also participate. The agent supplies the idea, the motif, even details which may not be at all in the conscious thought of the agent. The complicated structuring of the idea-pattern, the drama of the apparition, is the joint work of both the agent and the percipient.

Tyrrell is unable to accept either Myer's invasion theory or Gurney's theory of infection, judging that neither exhausts the possible theories of apparitions, although they lay the foundation on which any valid explanation must be based.

The primary condition essential to collective percipience seems

to be physical proximity. Why *physical* proximity should be important to a *nonphysical* function—particularly since such proximity is not necessary to the interaction of the primary percipient and the agent—is the puzzling question. Tyrrell solves the problem thus:

> The key to all the features which are introduced into the scheme is to be sought in the informing *motif*. The commonest theme, of course, resulting from the agent's state of mind at the moment of crisis, is the idea of the agent's own presence in the flesh in the neighbourhood of the percipient. The production of this theme is thorough. The figure is put into all the correct relations with its material surroundings. . . . Whatever would be the behaviour of the physical figure of the agent under existing circumstances is exactly reproduced. This thorough imitation of the physical event, which forms the *motif* of the theme, accounts, I believe, for collective percipience. Other people standing near the place where the apparition is seen would, if the apparition were a flesh-and-blood person, certainly see him. Therefore, to carry out the scheme thoroughly, they are so psychologically acted upon as to see the apparition and to see it appropriately according to their positions and distances. . . . Thus, collective percipience depends on the physical positions of the percipients, *not because physical facts have anything to do with it*, but because their physical positions bring them into the scheme of the idea-pattern. . . . Spectators, by their physical presence, become *relevant* to the theme of the apparitional idea-pattern and, because relevant, are drawn into it.

The general characteristics of the crisis-apparition and the ghost are the same. The differences lie, not in types of phenomena, but in the differently motivated idea-patterns. Both must be considered telepathic phenomena, but since there is some evidence (a case to the point is cited by Tyrrell) that sometimes ghost apparitions *may* seem to be dependent on a specific living agent, we must not overlook the possibility of mixed agency.

Pure Clairvoyance

The possibility that clairvoyance has an important role in veridical hallucinations is given careful attention by Tyrrell. While he does not deny that such a capacity *may* be called into play, he seems to doubt the necessity for assuming that it *does* so participate.

Tyrrell believes that it is theoretically important to determine whether *pure* clairvoyance in the strictest sense of the term is possible:

By no normal process can we achieve a state of direct awareness of a physical object; so that if there is such a thing as pure clairvoyance, it is our *only* possible means of becoming *directly* acquainted with the physical world.

The difficulty of isolating clairvoyance is recognized:

The question whether, in cases of clairvoyance, the subject ever gets to know facts which could not be obtained from *any* mind is debatable—and also important. If the evidence for precognition be admitted, it is not sufficient to show that the fact clairvoyantly cognised is known to no other mind at the time. It is also necessary to show that it *never will be known* to any other mind. Further, if retrocognition be a fact, it is also necessary to show that it *never has been known.* . . .

Tyrrell describes an experimental set-up, together with some details of an apparatus which might be adapted to this purpose, and which might be useful in trying to isolate clairvoyance. He believes that if positive results could be obtained by this method, pure clairvoyance would be unquestionably demonstrated.

Agency of Apparitions

Naturally, the agency of the cases in classes three and four (post-mortem and haunting or ghost-apparitions) has direct bearing on the survival hypothesis. After surveying the evidence for post-mortem apparitions and presenting cases displaying certain salient features, Tyrrell reaches the following conclusion:

. . . if we consider all *recognised* apparitions, belonging to the four classes, as being the expressions of telepathic idea-patterns originating with the persons represented by the apparitions, we have the explanation which most naturally accords with the evidence, and which also is most economical of hypotheses. But it involves the admission that some ghosts and post-mortem cases, and probably some post-mortem crisis-cases, are due to surviving agents. If we reject survivalist agency on grounds independent of the evidence of psychical research, we must split our explanations up as follows. Classes (1) and (2), up to crises at the moment of death, would be explained as originated by living agents. Class (2) after death up to a period of 12 hours (which is quite an arbitrary period) as originated by living agents, but with telepathic deferment. Class (3) as due to subjective hallucination on the part of the percipient, or percipients. Class (4) as due to some kind of physical traces left on the

material of the haunted house; or to retrocognition; or to some kind of persistent image, etc. The chief objections to the latter method seem to be that: (a) it introduces a good deal of complication; (b) it introduces sharp divisions into the explanations where no sharp divisions appear in the evidence; (c) the evidence for telepathic deferment is by no means good; (d) some of the cases in Class (3) cannot be due to purely subjective hallucination because the apparition represents a recognised figure which was unknown to the percipient. Also some of the cases are collective, and there is no independent evidence to show that subjective hallucinations are ever collective.

Speculation and Reflection

Tyrrell returns to his plea for the shifting of emphasis from *collection* of data to *organization* of fact and theory in accord with the encouragement given by Professor Price in his Presidential Address that we be "bold in speculation though careful in collecting facts." Holding that the great difficulty in obtaining public acceptance or even recognition of the work of psychical research to date lies, not in any insufficiency in the quantity or quality of the evidence, but in the psychological necessity for the individual to defend a creed of common-sense realism even in the face of psychic experiences of his own, Tyrrell believes that there is strong need for the re-education of the public and for the stimulation of a general awareness of the basic inadequacy of naïve realism as a foundation for interpreting even the more familiar aspects of human experience. Correlated with this plea for a revision of our means of understanding human phenomena is the awareness of the necessity for a more sophisticated statement of the problems of psychical research and a greater clarity and preciseness of definition pertaining to it. Particularly is this observation true in connection with the problem of survival; it is futile to keep asking questions in the frame-work of a meaningless philosophy. We do not know enough yet about the nature of personality to ask intelligent questions about the probability of survival. But it is by the growing understanding of the nature of personality which may be gained from the study of such phenomena as hallucinations that we may be enabled to state our questions in adequate terms and eventually may hope to be able to answer those questions.

For the world seems to have reached a stage in which belief in the value of the individual can no longer be sustained by the forces of religion and morality alone, but needs the backing of an intellectual conviction based on direct exploration of the human being.

Reviewer's Comments

The greatest contribution of Tyrrell's monograph is its insistent emphasis on the lawfulness of paranormal phenomena and its boldness in establishing these concepts on psychological rather than on physical grounds. Tyrrell's willingness to treat parapsychical phenomena in the terms of their own framework rather than to try to fit them into the setting of the physical sciences and his recognition of the causal efficacy of mind are counter-balanced by a realization that the physical sciences and the psychical sciences must eventually be reconciled in the mind of the public. This correlation of the two is not to be obtained by reducing one to the terms of the other. Rather, it may necessitate a far-reaching revision of many of the present popular concepts about both.

Tyrrell assumes that the "mid-level elements" which produce and express an apparition possess characteristics such as those McDougall described in his address. It is supposed that similar dynamics on the part of the agent are brought into play, regardless of whether the agent is a living person or a deceased "surviving personality." There seems to me to be some doubt whether McDougall, supposing *something* of the personality to survive, would attribute that power to the mid-level characteristics. In the address to which Tyrrell refers, McDougall stated:

> . . . there is the question of the bearing of this view [that the personality is composed of a hierarchy of monads varying in degree as to autonomy and selflike characteristic, with the conscious self at the head] upon the great problem . . . of life after death. Well, adoption of our hypothesis would necessitate a development of our view of the life after death of the body, one directly in line with the development . . . already undergone in the slow process of cultural evolution. . . . But now, I suggest, we have to make a further step and purge our conception still further. I, if I survive the dissolution of my bodily organism, shall, by our hypothesis, retain only those functions which I have not delegated but have developed by active exercise and those memories which are most truly mine [i.e., the memories which belong to the dominant monad, in

contrast to those of the less-conscious mid-level elements], the memories of my own activities.[3]

Subjectivity of Collective Hallucinations

A question may well be raised concerning Tyrrell's opinion that subjective hallucinations remain private and are never shared—that is, that subjective hallucinations are never collective hallucinations. He states that Gurney classifies as subjective hallucinations certain collective *post-mortem* appearances, but that Gurney himself admitted he could not be *sure* these cases actually were subjective. Neither do hypnotically induced hallucinations appear to be shared; but this field, according to Tyrrell, has not been exhaustively explored from the point of view of psychical research. With this minor concession, Tyrrell dismisses the whole subject of the possibility that a subjective apparition may be collectively perceived and concludes: "There is no evidence, so far as I am aware, that purely subjective hallucinations spread to others." But Gurney also classified as subjective certain cases of hallucinations in which the apparitions represented *living persons*.[4] A subjective origin also is very strongly suggested in what Gurney calls "rudimentary types" of hallucinations—hallucinations not having to do with any special human form or presence. These are usually experienced in the form of lights, which may vary in color or intensity, cloud-like formations, etc. Gurney describes at least five cases of this type which were collectively experienced.[5] He was reasonably sure that there was no physical explanation for them and that they were genuine hallucinations. Only one of the five cases seems to be in any way veridical; one percipient had an experience of this kind a few days after the death of her husband, and she interpreted it as having some kind of connection with him.

Case 316, from Gurney, is an instance of this kind. A husband and wife, while fully awake, saw a peculiar blue light. In describing it they wrote:

"We discussed it, and tried to fathom its meaning, over and over again, but could never arrive at any conclusion about it at all—except that as it

[3] McDougall, William. Presidential address. *Proc. S. P. R.*, 1920, **31**, 119-20.

[4] Gurney, E., Myers, F. W. H., and Podmore, F. *Phantasms of the Living*. London: Trübner and Co., 1886. See especially Vol. II, pp. 79 ff.; 190 ff.; 198, footnote; 224 ff.

[5] *Ibid.*, pp. 192 ff.

did not act as a forewarning to any coming event, and did not appear to serve any purpose at all, its appearance was utterly meaningless."[6]

It might be well to follow Gurney by defining the group as cases which do not apparently originate in any absent agent.

Some of the instances quoted by Tyrrell himself might well be subjective. Several of them, from all that can be deduced from the account he gives of them, seem to have no relation to any identifiable event or circumstance, either past or present. In some of these the figure in the apparition was neither recognized nor identified. The sole reason for considering them veridical seems to have been the fact that they were collectively perceived; and having accepted the fact of collective hallucination as sufficient reason for considering an apparition veridical, Tyrrell convinces himself that there is no evidence that subjective hallucinations spread to others.

Conditions Involved in Collective Hallucinations

There are a number of instances in which several percipients who have not been in the immediate vicinity of each other have shared in what seemed to be related hallucinatory experiences. In several of these cases the percipients had been "sharing the same life," a condition apparently favorable to such an experience. Here is evidence that physical proximity, even of the various percipients, is not fundamental.

On the other hand, there are a number of cases of multiple percipience in which the sharing of the same general experiences by the different percipients is less evident. The circumstances indicate that while the percipients may all have come within the psychological space[7] of the agent, it is possible that the psychological spaces of the percipients may not have overlapped. The percipients may be friends or acquaintances—that is, they may be in the psychological space—of the agent but may be entirely unknown to each other. The impulse from the agent may not be directed to one individual but may radiate out to several. It is not a message sent from the agent to the primary percipient, but is generally dispersed and available to anyone. We exercise a discriminatory function and pay attention to

[6] *Ibid.*, p. 193.

[7] Dr. Gardner Murphy, in his review of Tyrrell's article, uses Lewin's conception of psychological space in this connection. See *Jour. A.S.P.R.*, 1943, **37**, 160.

what is significant or interesting to us. When we are in the company of others, they may call our attention to something which might otherwise have no meaning for us; hence the apparition of a companion may be seen by those with him.

Multiple Hallucinations Not Discovered Until Later

I suggest that the statistics relating to collective hallucinations need further interpretation. It seems evident that being with some one else is not in itself a condition conducive to having an apparitional experience. Only twenty-four percent of the hallucinations reported by Tyrrell from the Census of Hallucinations were experienced in the presence of others. But of all those which were so experienced, about one-third were shared by some other member or members of the group. (This is not analogous to saying that one-third of the persons present were percipients. It would be interesting to know what proportion of the persons present saw the apparition, as well as how frequently some *one* in the group saw it.)

The proportion of cases in which the various percipients in the multiple-hallucination cases were not together or at least in the immediate vicinity of each other is very small. The accounts of these cases make it clear that the scarcity of such instances may not be due to the fact that so few of them occurred, but to the fact that the reporting of them is less likely to have been inadequate. A considerable number of the cases described as cases of a single hallucination may have been in reality undiscovered cases of multiple hallucination.

In many instances, the percipient, particularly if he believes himself to have had a unique experience, is reluctant to talk about what happened. Over and over, the records contain such remarks as: "It was not a matter I talked about"; or "I did not mention the circumstances to anyone." There is much less likelihood of a case of multiple percipience which has occurred when the percipients were together being mistaken for a case of single percipience. But even under this circumstance incidents have happened which were *almost* so interpreted. Case 320 from Gurney is an example. A certain Surgeon-Major Samuel Smith and his wife's mother, while engaged in playing music—she at the piano, he with a flute—experienced a

visual hallucination of an unidentifiable human figure. Mr. Smith wrote:

"I did not cease playing, and subsequently played other pieces by the old masters, sang some songs, and finally went to bed, and slept well. Nor did I mention the matter to my wife's mother that night, either at the time of the occurrence, or before retiring to rest. Now, however, comes the most remarkable part of the matter. At or about 11:30 a.m. on the following day, my wife's mother came into the private room, and suddenly said, 'Did you see something last night when you were playing?' I did not immediately reply, but the strange event of the preceding night flashed across my mind instantly. . . . I did not believe at the time that she had noticed the action on my part; and, as I have already said, I had not mentioned the matter to her, or even hinted at it."

" 'Why do you ask?' I replied.

" 'Because I thought you did.'

" 'Did you see anything?' I asked.

" 'Yes, I believed someone had come into the room, as I felt that someone had come in.' "[8]

It is also interesting to find in this case that the mother-in-law saw the figure from an angle which, in relation to the figure seen by the narrator, would have been *physically* impossible.

There are at least two cases of multiple percipience in which the fact that additional persons participated in the experience was not known for several years. One involved apparitions seen by two sisters living in the same house, but not in each other's company at the time of the experience.[9] Each saw the figure of a third sister near the time of her death. Neither confided in the other, but later, independently and after intervals of some years, both related their experiences to a niece. The story of the second sister's experience was not told until after the death of the first sister, she never having known that the sister who shared her home had experienced an apparition similar to hers.

The second of these cases was known from the start to have been multiply perceived, but the fact that there was a *third* participant, not present in the group, was not learned for approximately thirteen years.

[8] Gurney *et al.*, *op. cit.*, pp. 200-201. See also Vol. I, Case 169, p. 427.
[9] *Ibid.*, Vol. II, Case 653, p. 606.

Case 314. "July 29,1885. On Easter Wednesday, 1872, my sister-in-law, Mrs. W., sailed with her husband and three young children from Liverpool in the steamer, 'Sarmatian,' for Boston, U. S., where they arrived in due course and settled. In the following November she was seized with, and died from, suppressed small-pox, at that time raging in Boston. About the end of November, or the beginning of December in the same year, I was disturbed one morning before it was light, as near as may be between 5 and 6 a.m., by the appearance of a tall figure, in a long night-dress, bending over the bed. I distinctly recognised this figure to be no other than my sister-in-law, Mrs. W., who as I felt, distinctly touched me. My husband, who was beside me asleep at the time, neither saw nor felt anything.

"This appearance was also made to an aged aunt, residing at this time at Theydon Bois, near Epping, Essex. She is now alive, aged over 80 years, and residing at Hextable, near Dartford, in Kent. She is still in full possession of all her faculties. She told my husband as recently as the 4th. inst., that the appearance came to her in the form of a bright light from a dark corner of her bedroom in the early morning. It was so distinct that she not only recognised her neice, Mrs. W., but she actually noticed the needlework on her long night-dress! This appearance was also made to my husband's half-sister, at the time unmarried, and residing at Stanhope Gardens. The last named was the first to receive the announcement of the death of Mrs. W., in a letter from the widower dated December (day omitted), 1872, from 156, Eighth Street, South Boston, still preserved. The death was announced, among other papers (as my husband has recently learned), in the *Boston Herald*. A comparison of dates, as far as they could be made in two of the cases, served to show the appearance occurred after the same manner, and about the same time, *i.e.*, at the time of, or shortly after, the death of the deceased. Neither myself nor the aged Mrs. B., nor my husband's half-sister, have experienced any appearance of the kind before or since. It is only recently, when my husband applied to his half-sister to hunt up the Boston letter, that we learnt for the first time of this *third* appearance."[10]

Still further consideration might be given to the possibility that an agent other than the figure appearing in the apparition, is responsible for the apparition. Tyrrell explicitly recognizes this possibility; any figure may be hallucinated, depending on the idea to be conveyed. Secondary figures may accompany the primary figure, that of the agent responsible for the apparition. Also, as demon-

[10] *Ibid.*, Vol. II, p. 181. See also Case 313, pp. 179-80.

strated by one experimental case which Tyrrell describes, the agent may intentionally cause the apparition of a deceased person, unaccompanied by any apparition of the agent himself, to appear to both the person intended to be the percipient and to a companion who, without the agent's knowledge, happened to be with him. Tyrrell further cites an instance, again in one of the experimental cases, in which the agent succeeded by design in initiating an hallucination of a nonhuman apparition, causing the percipient to see luminous clouds by concentrating on a bright disc himself.

In all the foregoing cases, Tyrrell naturally attributes the fact that something other than the figure of the agent was seen to the fact that it was the *intention* of the agent not to be seen. He further states that it seems likely that we must assign certain spontaneous phenomena of the poltergeist type to living agents, and points to a few instances in which these apparently were dependent on the presence of a specific individual. Likewise, some of the epidemic appearances of traditional ghosts peculiar to certain localities might be the result of collective idea-patterns in the minds of local residents. But Tyrrell is by no means ready to attribute all haunting phenomena to the unconscious agency of living persons. Certainly I would not suggest that they should be so explained. There are, however, a few more cases which Tyrrell does not consider which might be given due attention in connection with this subject. These are the spontaneous cases, not in any way experimental, in which it is possible that the figure seen in the apparition was not that of the agent responsible for originating it.

There are three spontaneous cases which in Gurney's opinion are probably instances wherein the figure in the apparition did not represent the agent of the hallucination.[11] None of these cases offers clear-cut evidence that the agent *was not* represented by the apparitional figure; rather, they strongly indicate that the agent *may have been* some person other than the one represented by the apparitional figure. The most striking instance of this sort, quoted from Gurney, may be classified as belonging to the crisis-apparition variety.

The following case is a sort of comedy of errors. Only two of the four hallucinations which it includes represented the absent agent; as to

[11] *Ibid.*, Vol. II. See Case 311, pp. 176 ff.; Case 348, pp. 239 ff.; Case 652 p. 605.

the two which did not, I shall hazard no further supposition than that their coincidence with the others was not accidental. . . .

"Captain Robert Fagan, late of the Bengal Artillery, while in charge of the bridge of boats at Lahore, was in the district on the river collecting boats. One morning, during his absence from home, his eldest boy, of about 6 years old, seeing his mother just dressed for breakfast in a coloured muslin, begged her to put on a black dress, saying, 'Because papa is dead.' The mother, after diverting his thoughts for a short time, said, 'Shall I put on a black dress now, Charlie?' 'Oh, no,' he answered, 'papa is not dead now,' and ran away.

"On leaving her room, she was met by the head nurse, a Scotch-woman, with the inquiry if she had heard from the master that morning. When told his usual letter had not come, she said, 'Something very uncanny has happened to him, for looking out of the window just now, I saw Annie, the undernurse, and the gardner go up to master's favourite rose-tree and gather a flower, and before she could have got in from the garden, I found her in the night nursery, which she had never left, finishing bathing the children.'

"Not thinking much of this, Mrs. Fagan passed on to the breakfast-room, where she expected to find her visitors, Captain and Mrs. Reveley. Not doing so, she went to Mrs. R.'s room, whom she found still at her toilet, for which unpunctuality Mrs. R. apologised, saying she had had a dreadful fright, having seen Mrs. Fagan standing in front of the chest of drawers, who, when asked how she had come unobserved into the room, turned around and then deliberately vanished through the chest of drawers and the door behind it.

"This third strange remark led Mrs. Fagan to relate all three at the breakfast-table to Captain Reveley. She could not help observing how unlike his usual manner was his brusqueness in cutting short the conversation, as soon as he had heard all particulars. Five days passed without any information—private or official—from Captain Fagan; but at the end of that time he arrived home looking ill, and saying that, on the morning of which we have been speaking, he was with difficulty resuscitated from drowning, the boat in which he was having capsized. This was naturally taken as the solution of the mystery. Captain Reveley, turning to Mrs. Fagan, said, 'I must apologise for my seeming brusqueness of manner that morning, but I feared to alarm you by seeming to attach any importance to what happened, and lest I should be induced to tell you of the greater fright I had myself had than any of you. For, Fagan,' addressing the Captain, 'as I passed from your office, where I had been reading with

the Moonshee, and going through the drawing-room, I distinctly saw you sitting in your usual chair.' "[12]

Tyrrell offers considerable argument that many cases of haunting should be considered just what they purport to be. He cites a famous case, that of the Morton ghost, as an example of a type of phenomenon far more developed and complicated than the more rudimentary and unintelligent expressions which in another case were attributed to a living agent. Tyrrell insists that while the various percipients contributed something to the creation of the apparition of the Morton ghost, the theme cannot have originated with them. The effort on the part of a hypothetical living agent in trying night after night over a period of seven years to make the figure appear would have been prodigious and would seem to be relatively unrewarded. That explanation does not seem plausible to Tyrrell.

There is one statement which was made by Miss Morton and quoted by Tyrrell which causes me to wonder if Miss Morton herself may not have been the agent: "Miss Morton says, 'I felt conscious of a feeling of *loss* as if I had lost power to the figure.' " She might have been entirely unconscious of the role she was playing.

I do not believe we have sufficient evidence yet to allow us to speculate (as Tyrrell does) about the *amount* of effort (or the difficulty of sustaining that effort over a long period of time) which it is necessary for the agent to exert to produce an apparition. Granted that the effort extended in *experimental* cases where the agent *consciously* tries to project an image is considerable, it does not necessarily follow that the hypothetical mid-level functions are not more economical in their expenditure of energy to achieve the same results.

It is to be hoped that Tyrrell's suggestion of an experimental procedure for isolating pure clairvoyance will be adopted—better, that Tyrrell himself will carry out his own plans along this line. I confess that I do not fully appreciate the particular theoretical importance he attaches to it.[13] To him it is highly significant because

[12] *Ibid.*, Vol. II, Case 652, pp. 605-6.

[13] The reader may wish to consider the "ESP shuffle" research in the light of Tyrrell's comments on clairvoyance. (See this JOURNAL, Vol. 2, pp. 119-31.) It should afford an interesting approximation to his requirements. There is the more recent argument that extrasensory perception of objects (that is, clairvoyance) is necessary to explain the working of the PK effect. For a statement of this point, see this JOURNAL, Vol. 7, pp. 39-40.—Ed.

"by no normal process can we achieve a state of direct awareness of a physical object; so that if there is such a thing as pure clairvoyance, it is our *only* possible means of becoming *directly* acquainted with the physical world." He has already pointed out that "we know physical objects only through the intermediary of our own sense-data, and these sense-data we create ourselves." Obviously our clairvoyantly-obtained knowledge must also be presented to our conscious mind in the form of sense-data, which we also create ourselves. The only difference seems to be that in the case of sensory perception the sense-data are brought into play by some physical stimulus, while in clairvoyance they are brought into play by some as-yet-unknown but presumably nonphysical stimulus. I am at a loss to see how the ensuing knowledge is any more *direct* in the one instance than in the other.

GLOSSARY

In order to avoid constant redefining of commonly recurring terms in papers appearing in this JOURNAL, the following definitions are submitted for convenient reference. Words defined elsewhere in the glossary are italicized in the text of the definitions.

*For a simple description of those terms marked by an asterisk, as they apply to the ESP test data, see Chapter VIII and the Appendix of *A Handbook for Testing Extra-Sensory Perception* by C. E. Stuart and J. G. Pratt. A mimeographed copy of the relevant pages will be sent on request to subscribers who do not have the book mentioned. Further explanation may be found in any elementary statistical text.

AGENT: In tests for *telepathy*, the person whose mental states are to be apprehended by the *percipient*. In *GESP* tests, the person who looks at the *stimulus object*.

AVERAGE SCORE: Average number of *hits* per *run*.

BM (BLIND MATCHING): The technique in which the *subject matches* a *deck* of *ESP cards* to five *key cards* which are laid out face-down before him in an unknown order. Unless otherwise stated, the order is also unknown to the experimenter.

BT (BEFORE TOUCHING): The technique in which the top card of the face-down *deck* is *called* and, after being called, is laid aside for *checking* at the end of the *run*. Each card in the *deck* is treated in the same way.

CALL v.: To attempt to identify a *target* or *stimulus object* (or mental state of an *agent* in *telepathy*).

CALL n.: The *response* described above; also the resulting selection.

CHANCE:* The complex of undefined causal factors irrelevant to the purpose at hand.

CHANCE EXPECTATION = MEAN CHANCE EXPECTATION: The most likely *score* if only *chance* obtains.

CHANCE AVERAGE: *Mean chance expectation* in terms of *average* per *run*.

CHECK: To determine a *score* after the completion of a *run* by comparing the order of the *subject's calls* with the order of cards in the deck.

CHI-SQUARE: A sum of quantities each of which is a *deviation* squared divided by an expected value. Also a sum of the squares of *CR's*. (Occasionally the square of a simple CR may be used as chi-square.)

CLAIRVOYANCE: *Extrasensory perception* of objective events as distinguished from *telepathic* perception (of the mental or subjective events of another person).

COVARIATION: Correlation evaluated in terms of theoretical means and *standard deviations*.

CR (CRITICAL RATIO):* A measure to determine whether or not the observed *deviation* is *significantly* greater than the expected random fluctuation about the *average*. The CR is obtained by dividing the observed *deviation* by the *standard deviation*. (The *probability* of a given CR may be obtained by consulting tables of the probability integral, such as Pearson's.)

CR OF THE DIFFERENCE: The observed difference between the *score averages* of two samples of data divided by the *standard deviation of the difference*. (Where the samples to be compared are of equal number of runs, the difference between total *hits* may be divided by the *SD* of the total number of *runs* of both samples.)

DECK: Twenty-five *ESP cards*, five of each suit.

DEVIATION:* The amount an observed number of *hits* or an *average score* varies from the *mean chance expectation* or *chance average*. A *deviation* may be total (for a series of *runs*) or average (per *run*).

DIE-THROW: The throwing or mechanical release of a single die regardless of the number thrown at the same time.

DT (DOWN THROUGH): The technique in which the cards are *called* down through the *deck* before any are removed or *checked*.

EMPIRICAL CONTROL: An experiment which wholly or partially follows the main experiment with the exception that the conditions are designed to exclude the possibility of *ESP*.

ESP (EXTRASENSORY PERCEPTION): Response to an external event (perception) not presented to any known sense.

ESP CARDS: Cards, each bearing one of the following five symbols: star, circle, three parallel wavy lines (called "waves"), square, plus.

ESP SYMBOLS: See plate opposite page 1, this JOURNAL, Vol I, March, 1937.

ESP TESTS: A considerable number of techniques come under this heading which are conveniently represented by initials, the principal ones being: *BT, DT, PT, GESP, BM, OM, STM*.

EXPECTATION; see CHANCE.

EXTRACHANCE: Not due to *chance* alone.

FREE MATERIAL: *Stimulus objects* that are not limited to a known number of categories.

GESP (GENERAL EXTRASENSORY PERCEPTION): A technique designed to test the occurrence of *extrasensory perception*, permitting either *telepathy* or *clairvoyance* or both to operate.

HIT: The correct correspondence of a *subject's call* or response with a *stimulus card* or *object*.

HIT FREQUENCY DISTRIBUTION: The grouping of the total *hits* in a *series* of *runs* with respect to their original position in the *run*.

KEY CARD: One of the five cards (where there are five suits) against which the cards of the test *deck* (i.e., *target cards*) in the *matching* tests (*OM*, *BM*, *STM*, etc.) are *matched*.

MATCHING: A form of *calling* in which a *target card* is placed opposite the *key card* which the *subject* selects to identify it. Also, in the evaluation of *free material*, the act of a judge in identifying a given *response* with a *stimulus object*.

MEAN CHANCE EXPECTATION; see CHANCE.

OM (OPEN MATCHING): The technique in which a *subject matches* a *deck* of *ESP cards* to five *key cards* which are face-up before him.

P (PROBABILITY):* A mathematical estimate of the expected relative frequency of a given event if chance alone were operative.

PARAPSYCHOLOGY: A division of psychology dealing with the paranormal—those psychical effects which appear not to fall within the scope of what is at present normal and recognized law.

PERCIPIENT: The person who makes the *calls* in a test situation.

PK (PSYCHOKINESIS): The direct influence exerted on a physical system by a *subject* without any known intermediate energy or instrumentation.

RESPONSE: The act of the *subject* in attempting to identify the *stimulus object*.

RSR (RUN SALIENCE RATIO): A measure of *salience* within the *run*.

RUN: A succession of *trials*, usually the *calling* of a *deck* of 25 *ESP cards* or symbols. In *PK tests*, 24 single *die-throws* regardless of the number of dice thrown at the same time.

SALIENCE: The relation of rate of success in the end *segments* of the *run* to that of the middle *segments*; also the relation of the rate of success in the end *trials* of the *segment* to that of the middle *trials*.

TERMINAL SALIENCE: A higher rate of *deviation* in the end *segments* of the *run* (or in the end *trials* of the *segment*) than in the middle *segments* (or *trials*).

MIDDLE SALIENCE: A higher rate of *deviation* in the middle *segments* of the *run* (or in the middle *trials* of the *segment*) than in the end *segments* (or *trials*).

SCORE: The number of hits made in one *run.*

TOTAL SCORE: *Score* of any number of *runs.*

AVERAGE SCORE: *Total score* divided by number of *runs.*

SCREEN: An opaque barrier used between the *subject* and the card or *agent.* The main types of screens are illustrated in this JOURNAL on their first introduction in print.

SD (STANDARD DEVIATION):* The theoretical root mean square of the *deviations.* It is obtained from the formula $\sqrt{npq}$, in which n is the number of single *trials*, p the *probability* of success per *trial*, and q the *probability* of failure. (For *ESP cards*, $SD = 2\sqrt{}$ no. of *runs.*)

SD OF THE DIFFERENCE: For both *ESP cards* and *PK tests* using dice, the *SD* of the difference is equal to $\sigma_s \sqrt{1/R_1 + 1/R_2}$ where σ_s is the *SD* of a single *run* and R_1 and R_2 are the number of *runs* in the respective samples compared. This gives the *SD* of the difference for *run score averages.*

SEGMENT: One of the five consecutive sets of five *calls* in a *run* of 25 *trials.* The first five *calls* would constitute the first *segment;* the second five, the second, etc.

SERIES: Several *runs* that are grouped in accordance with a stated principle.

SIGNIFICANCE:* A numerical result is significant when it equals or surpasses some criterion of degree of chance improbability. Common criteria are: a probability value of .01 or less, or a *deviation* in the expected direction such that the *critical ratio* is 2.5 or greater.

SR (SALIENCE RATIO): A measure of the relation of the rate of success in the end *segments* of the *run* (or in the end *trials* of the *segment*) and that of the middle *segments* (or *trials*). (For details of the manner of obtaining SR's, see Vol. 5, pp. 193-195.)

SSR (SEGMENTAL SALIENCE RATIO): A measure of *salience* within the *segments* of the *run.*

STIMULUS OBJECT: The *ESP card* or drawing or other object, some identifying characteristic of which is to be apprehended by the *subject.*

STM (SCREENED TOUCH MATCHING): The technique in which the *subject* makes his *call* by pointing to one of five positions or exposed *symbols* under a special *screen.* The experimenter places the *target card* so designated in the position pointed to. The *screen* blocks all vision by the *subject* of the *cards* and their manipulation by the experimenter.

SUBJECT: The person who is experimented upon. Most commonly the *percipient* in *ESP*, though also the *agent* in *telepathy*.

TARGET: In *ESP tests*, the *stimulus object*. In *PK tests*, the faces of the die (or combination of faces) which the *subject* attempts to bring up in the act of throwing.

TARGET CARD: The *card* which the *percipient* is attempting to perceive (i.e., to identify or otherwise indicate a knowledge of).

TARGET DECK: The *deck* of cards the order of which the *subject* is attempting to identify.

TELEPATHY: *Extrasensory perception* of the mental activities of another person. It does not include the *clairvoyant* perception of objective events.

TRIAL: A single attempt to identify a *stimulus object*.